THE CAMBRIDGE HISTORY OF PHILOSOPHY OF THE SCIENTIFIC REVOLUTION

EDITED BY
DAVID MARSHALL MILLER
AND DANA JALOBEANU

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The early modern era produced the Scientific Revolution, which originated our present understanding of the natural world. Concurrently, philosophers established the conceptual foundations of modernity. This rich and comprehensive volume surveys and illuminates the numerous and complicated interconnections between philosophical and scientific thought as both were radically transformed from the late sixteenth to the mid-eighteenth century. The chapters explore reciprocal influences between philosophy and physics, astronomy, mathematics, medicine, and other disciplines, and show how thinkers responded to an immense range of intellectual, material, and institutional influences. The volume offers a unique perspicuity, viewing the entire landscape of early modern philosophy and science, and also marks an epoch in contemporary scholarship, surveying recent contributions and suggesting future investigations for the next generation of scholars and students.

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Preface

This addition to the Cambridge Histories is the brainchild of Hilary Gaskin of Cambridge University Press, who saw an opportunity to consolidate an important body of scholarship and add an accompaniment to Daniel Garber and Michael Ayers's *The Cambridge History of Seventeenth-Century Philosophy*. We hope we have not abused her vision or her forbearance, even if the project outstripped deadline after deadline. Many colleagues helped plan the structure and contents of the volume, though conversations with Daniel Garber and Marius Stan were especially instrumental.

In July, 2018, many of the book's authors gathered to discuss their contributions at a conference hosted by Iowa State University and supported by a symposium grant from the university's Center for Excellence in the Arts and Humanities and a Collaborative Research award from the National Endowment for the Humanities (RZ25567217). We are grateful for that support, though any views, findings, conclusions, or recommendations expressed in this book do not necessarily represent those of the National Endowment for the Humanities.

Other workshops and colloquia helped shape the volume. We would especially like to thank Niccolò Guicciardini for organizing a workshop on *The History of Philosophy of the Scientific Revolution* at the University of Bergamo in October, 2017. Subsequent workshops at the Institute for Research in the Humanities–ICUB in Bucharest brought together some of the authors in 2018 and 2019. The framework of the *Princeton–Bucharest Seminar in Early Modern Philosophy* also significantly assisted the completion of the project.

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Introduction

The Disciplinary Revolutions of Early Modern Philosophy and Science

DAVID MARSHALL MILLER AND DANA JALOBEANU

The period stretching from roughly the middle of the sixteenth century to the middle of the eighteenth witnessed foundational changes in the intellectual culture of western Europe. Among these developments were many new ideas – some appearing on the scene fully grown and difficult to overlook, sparking controversy or enthusiastic assent; some, more modest at first, growing steadily over subsequent years; and still others, initially invisible, yet persisting and sometimes erupting with dramatic effects. Eventually, these novel views coalesced into the intellectual traditions that have constituted the early modern canon. Of the myriad transformations involved in this period of ebullition, two were central. The first was an explicit reformation: Philosophers proclaimed the necessity to set aside the preoccupations of their predecessors in order to "start again right from the foundations."[1] The second was less declarative, but more profound: The explanations used to account for natural phenomena were radically revised.

Contemporary scholars have recognized the significance of these developments in the periodization used to study them. The "early modern period" is distinguished (sometimes sharply) from the "late Renaissance" that preceded it; and the "early moderns" are often seen as our direct ancestors, whose thought is much more connected to the present than that of previous eras. Historians of philosophy refer to "early modern philosophy" that departed from medieval and Renaissance precedents, especially Aristotelian Scholasticism. Historians of science even more dramatically refer to the "Scientific Revolution" that replaced various domains of natural knowledge – natural philosophy, the mathematical "mixed sciences," natural history, medicine, alchemy, and so on – with the modern amalgamation we simply (and anachronistically) call 'science'.

[1] Descartes (1984–1991, 2:12).

These transformations of philosophy and the sciences overlapped, and many figures – including René Descartes, Galileo Galilei, Francis Bacon, Pierre Gassendi, Robert Boyle, and Isaac Newton – feature prominently in both histories of philosophy and histories of science. Nevertheless, scholars traditionally have treated philosophy and the sciences separately, as the subjects of distinct disciplinary fields with radically different historiographical approaches. The chapters in this volume represent a countervailing trend of the last three decades that views the intellectual history of the early modern period as an integral whole, wherein "science" and "philosophy" cannot be readily pulled apart, not least because the historical authors themselves did not recognize the distinction. From this perspective, one sees that the emergence of modern philosophy and that of modern science were not separate phenomena, but facets of the same transformations, taking place in the same period, in texts often written by the same authors. The history of philosophy of the Scientific Revolution is inextricable from the history of early modern philosophy.

Essential Tensions

Recognizing the fundamental unity of early modern thought has led scholars to re-evaluate classic debates and traditional accounts, but it also has been accompanied by methodological difficulties. Disciplinary divisions are more persistent than we like to think. While historians of early modern philosophy have distanced themselves from the presentist intellectualism of older histories of philosophy, they have not embraced the contextualist social constructivism found in much history of science. As a result, the analyses in this book occupy a still unsteady middle ground. Whereas the individual chapters present the lessons of recent research, our overarching editorial aim has been to knit them together into a comprehensive story that acknowledges but also attempts to balance their methodological tensions. This editorial outlook gives structure to the volume, and deserves explication here.

The Contextualist Turn

A couple of generations ago, historians of philosophy in general, and scholars of early modern philosophy in particular, mined the work of past authors for material relevant to contemporary concerns. Early modern writers like Descartes and John Locke were made to speak to present-day debates in metaphysics, epistemology, ethics, and so on. Yet by conceiving historical and modern figures alike as participants in an ahistorical discourse, this

approach tends to analyze past thought according to present categorizations. It also encourages attempts to reconcile the various parts of an author's corpus into a unified, coherent system, and even to "rationally reconstruct" the best version of a philosopher's view – to elaborate what, e.g., Descartes *should* have said, even if he actually did not. These systematized versions of philosophers' thought are most valuable for solving current philosophical problems, but this method treats philosophical arguments, whatever their provenance, as disembodied, timeless, and unchanging. It removes texts from their contexts.[2]

In the 1990s, historians of early modern philosophy began to turn away from such attempts to reconstruct maximally coherent interpretations of texts. They became far more sensitive to the contexts in which those texts were produced.[3] In the years since, the distinction between text and context has been further blurred, as a new generation of scholars have realized that the traditional historiographies are not only insufficient, but often also misleading. Anachronistic categorizations, it has become clear, have obscured the rich and dynamic interactions between different kinds of thinkers and their ideas. The Descartes and the Lockes of the period developed their view in dialogue with a range of interlocutors and concerns that cannot be neatly delineated by the familiar (to us) categories of metaphysics, epistemology, and even philosophy.

This "contextualist revolution"[4] in the study of early modern philosophy has entailed engagement with the history of the Scientific Revolution. The development of early modern philosophy was intertwined with the radical transformations in the understanding of natural phenomena produced by figures such as Nicolaus Copernicus, William Gilbert, Galileo Galilei, William Harvey, and others. It has become clear that one cannot properly understand Descartes, for instance, without taking into account his interactions with the mathematicians and "scientists" of his day; his sensitivity to the events of the "Galileo Affair"; or his reading of and correspondence with figures hitherto considered as belonging to the history of medicine. Hence, awareness of the history of science has become a necessary condition for contextualized readings of early modern philosophers. Indeed, such work has

[2] Jonathan Bennett is an archetype of the older approach. See Bennett (1971); Bennett (1984). An updated version is offered by Della Rocca (2015).

[3] Daniel Garber (especially Garber 1992) has been a standard-bearer. The papers in Garber and Ayers (1998) helped originate the trend. See also his responses to Bennett and Della Rocca (Garber 2013b; Garber 2015b).

[4] Mercer (2019).

shown that the stark separation of "philosophy" and "science" itself is an anachronistic imposition of latter-day conceptualizations.[5]

Gains and Losses: Abandoning Methodological Consensus

The contextualist turn has produced concrete gains. Sharpened sensitivity to how historical actors characterized their own practice has yielded a more sophisticated understanding of the extremely complex landscape of early modern intellectual life. We have learned much about the scientific work of those usually considered philosophers,[6] and about the philosophical contributions of "scientists."[7] Recent scholarship has encompassed a remarkable expanse of early modern theoretical disciplines – both scientific and philosophical. It has shown that "natural philosophy" was not the only important antecedent of modern science and philosophy. Pursuits formerly considered marginal, such as medicine and alchemy, have also been sounded for philosophical significance.[8]

However, the contextualist turn has had as an (unintended) consequence the dissolution of the field's methodological coherence. Recent studies, though successful individually, have become particularized – they do not share basic categorizations, aims, or vocabularies. It is therefore difficult to compile them into a general account, and this, in turn, causes problems for new scholarship and pedagogy. Whereas, for instance, it might have once been easy to place Descartes among the "Rationalists" and Locke among the "Empiricists" (and to teach courses organized along those lines), the realization that these categories are not historically valid has made it hard to say how the figures stand in relation to one another in the roiling intellectual landscape of the period. Likewise, the philosophical canon has been destabilized – a positive development, but one leading to hard choices. If Newton, for one, has earned a place in courses in the history of philosophy, which figure currently taught must make way? Even if the contextualist cause is ascendant, the "revolution" is not complete.[9]

[5] Studies that mix early modern science and philosophy are now common. See, e.g., Sorell, et al. (2010); Dobre and Nyden (2014); Garber and Roux (2013); Biener and Schliesser (2014).

[6] In addition to Garber (1992), see, e.g., Sasaki (2003); Anstey (2011b); Smith (2011).

[7] Newton is the foremost example of a scientist made philosophical (Janiak 2008; Harper 2011; Ducheyne 2012; Biener and Schliesser 2014). Robert Boyle and Francis Bacon have undergone similar reconsideration (Anstey 2000b; Gaukroger 2001; Hunter 2015; Jalobeanu 2015; Anstey 2018; Jalobeanu 2019). Others, like Émilie Du Châtelet, are poised to receive similar treatment (Zinsser and Hayes 2006; Hagengruber 2012).

[8] E.g., Clucas (2011); Nachtomy and Smith (2014); Distelzweig, et al. (2016).

[9] Christia Mercer declares victory in the contextualists' revolution, but admits that the "differences" among current historians of early modern philosophy "reside only in the projects selected and skills used to attain their goal" (Mercer 2019, 529). The 'only' here

The kernel of these difficulties is the fundamental opposition of analysis and narrative. The conceptual analysis needed to explicate a philosophical argument and the historical narrative needed to describe its development through time require contrary presumptions. Analysis supposes a theory's stability and independence; narrative supposes its malleability and dependence on antecedent conditions. In order to avoid the dilemma, scholars have tended toward the extremes. Historians of philosophy, at least in the anglophone world,[10] gravitate toward analysis, which brings a range of consequences. First, there is the tendency, noted above, to isolate texts from contexts and seek coherence within a corpus, so as to serve contemporary interests. This is attended by limiting attention to the narrow scope of canonical figures thought to be most informative on those issues. Second, since historical precedents are pushed to the background, historians of philosophy tend to view each historical figure as an innovator, without antecedent. This is especially true of the early modern period and the Scientific Revolution, which are seen (to adopt a geological metaphor) as catastrophic breaks with the past. Third, there is the inclination to restrict explanations to intellectual considerations. This is partly in the nature of philosophy, insofar as it is the study of reasoning, but it also stems from the muting of context, where social and cultural factors can play a significant role. History of philosophy inclines toward textual internalism, presentism, catastrophism, and intellectualism.

Scholarship in history of science provides an instructive contrast, since it gravitates toward narrative at the opposite extreme. There, the aim is less to draw lessons for present-day application than to get the history "as it really was." One finds much more emphasis on contextual factors, including material conditions and social roles, and there is far less adherence to a canon. This approach stresses the continuity of intellectual development, to the point that it has become standard to question the existence of a Scientific Revolution in the first place. History of science, that is, tends toward externalism, antiquarianism, uniformitarianism, and socio-cultural reductivism.[11] The contrasting features of history of

elides the many problems we emphasize. Also the "selection" of projects is typically motivated by non-contextual, presentist concerns. See, e.g., the historiographical wrangling in (Lærke, et al. 2013).

[10] Scholars in Europe and elsewhere have always been more sensitive to context, so the recent shift has not been as dramatic there.

[11] The literature on historiography of science is extensive. For a sampling, especially regarding the status of the Scientific Revolution, see Shapin (1992); Cohen (1994); Shapin (1996); Harrison (2007); Smith (2009).

philosophy and history of science are not binary – instead, they indicate spectra of historiographical approaches. But even if we over-generalize, the comparison illustrates the divergences motivated by the core analysis–narrative opposition.

Contextualism in the history of philosophy moves away from the traditional analytical extreme. But this has raised the problems of reconciliation that the extreme position avoids. How to combine analysis and narrative? How much of the context is explanatory – i.e., where does one draw the limits of relevance? Are social and political factors important, or can explanations be given solely in terms of intellectual motivations? Which figures deserve attention – how much should the canon be "exploded"? Does one tell stories in terms of continuous traditions or of sudden innovations?[12] Is one to use only actors' categories, or are anachronistic descriptions permissible? As the reader will discover in the following pages, a thoroughgoing consensus on such methodological questions has not yet emerged. Each of the contributors to this volume answers them in different ways. Some are closer to the analytical extreme of each spectrum, others to the narrative.

Incidentally, the contextualist turn, with the resulting methodological uncertainty, has been mirrored in the adjacent realm of philosophy of science, where an initial enthusiasm for the "integration" of analytical philosophy of science with narrative history of science has been frustrated by uncertainty about how to effect it.[13] This, too, has led to an array of disparate approaches that do not entirely cohere.[14] Though this volume is intended as a contribution to the history of philosophy, not to philosophy of science or history of science, it does intersect history of philosophy with the history of science. Moreover, its editors and many of its authors are practitioners of history and philosophy of science. Consequently, salient issues will inevitably arise, and historians and philosophers of science will find useful material for their own attempts to comprehend the Scientific Revolution. Likewise, the methodological strategies used here will translate to work there.

[12] Thomas Kuhn called this *the* "essential tension" (Kuhn 1977a).

[13] See Domski and Dickson (2010); Mauskopf and Schmaltz (2012); Herring, et al. (2019).

[14] Compare, for instance, the generally more analytical tendencies in scholarship associated with the Committee for Integrated History and Philosophy of Science (known as &HPS), where historical cases tend to serve presentist concerns, with the more narrative scholarship of the Society for the History of Philosophy of Science (HOPOS). For the former, see Arabatzis and Howard (2015); Stadler (2017); for the latter, see the Society's organ, *HOPOS*.

Disciplinary Histories

The lack of methodological consensus among recent historians of early modern philosophy poses an editorial problem: how should a book such as this be organized? Our solution has been to adopt *discipline* as the unit of analysis. This aligns with a typical strategy of investigation, which begins by analytically delineating a phenomenon to be examined, and then constructs a narrative carefully following the chains of influences that led to it or followed from it. Drawing these connections leads naturally to an examination of the features by which early modern thinkers identified themselves with peers or distinguished themselves from foes – the features, that is, that separate one discipline from another. By tracing these disciplinary boundaries, scholars become attuned not just to the argumentative positions historical authors have taken, but also to the alternative views to which they were responding. This also comports with an increased sensitivity to contextual factors, since early modern disciplines could be organized in many ways – they were not just areas of inquiry or theoretical commitments, but also social institutions (schools, sects, clubs, academies, etc.), conceptions of teaching and learning, and methods of knowledge production and administration. Even if they broadly agreed with one another, authors could end up on different sides of a disciplinary boundary. Histories that use disciplines as units of analysis accommodate these contextual considerations.

Once one adopts disciplinary history as a methodology, the story of the early modern period becomes one about the multiplication and reorganization of intellectual disciplines. Surveying the intellectual landscape of the period, one observes the emergence of a number of new disciplines around a variety of problematics and projects. Especially productive were moments where existing disciplines came into contact with one another – either antagonistically or cooperatively. In these situations, the competing ideologies and methodologies were called into question, so that resolving the impasse often created a new discipline, as authors drew together the tenets and methods of the previously disparate fields. One example is when mathematicians staked claims to the causal reasoning of natural philosophers, spawning various forms of "physico-mathematics" that eventually led to classical mechanics.[15] Another is when logicians attempted to import the curative methods of medicine into reasoning, generating a novel "art of thinking."[16]

[15] See especially the chapter by Van Dyck in this volume (ch. 14), and also those by Miller (ch. 17) and by Omodeo and Regier (ch. 13).

[16] See the chapter by Corneanu and Vermeir (ch. 8). Related issues regarding psychology are taken up in the chapter by Pécharman (ch. 19), and the one by Hamou (ch. 18) describes how instruments were thought to "cure" the senses.

From a retrospective, analytic viewpoint, one sees here the early modern germination of the novel ideas that produced our modern world. From above, so to speak, these "inflection points" look like new directions of thought – solitary green shoots in an empty field. But from below, taking a contemporary, narrative perspective, one sees a continuous tumult of activity. The ground is saturated with intra- and interdisciplinary disputes, and from this frisson grow new ideas and new traditions. Much of the activity is obscure to us only because it is about issues that no longer concern us.

Note that the contextualists' synthetic, middle-of-the-road approach de-emphasizes the notion of the "Scientific Revolution," but does not entirely efface it. Unlike more traditional histories, the "Scientific Revolution" is not identified with a thematic arc, such as "mechanization of the world picture" or "invention of science," that provides the armature upon which the narrative is constructed.[17] And yet careful attention reveals that the early moderns themselves thought something radically new was afoot, even if they could not adequately express exactly what it was. Once again, contextualists illuminate how the historical authors themselves set up dialectical oppositions between old and new. Consequently, instead of asking whether there was a "Scientific Revolution," they ask what 'new astronomy' meant for Johannes Kepler, what 'new science' meant for Galileo, what 'great instauration' meant for Francis Bacon, and so on. In this way, 'Scientific Revolution' becomes an abstract, generic term comprising all the specific, concrete innovations of the early modern period. Its vagueness, though, is not problematic, since it is not itself the focus of study. We need not get hung up debating its reality or its temporal bounds.

It is also worth noting what this historiographical approach does not do. For one thing, it eschews the imposition of latter-day ideological categorizations. "Rationalism" and "Empiricism" – so central to traditional history of philosophy – are not meaningful conceptualizations here. For another, it does not explode the canon, but rather historicizes it. If one carefully follows the disciplinary dialectics, one finds that certain figures play outsized roles. They show up again and again as authorities whose support lends credence to a position, or as *bêtes-noires* whose support renders a position suspect. Sometimes these figures are those who have long occupied the spotlight, like Descartes; sometimes they are those who have hitherto lurked in shadow, like David Gorlaeus.[18] Thus, scholars in this vein have not dispensed

[17] This is a feature both of older work, such as Dijksterhuis (1961); Westfall (1971a), and newer histories, like Gaukroger (2006); Wootton (2015).

[18] See the chapters by Garber (ch. 2) and Hattab (ch. 3) in this volume.

with the canon entirely. Their studies remain centered on a group of figures thought to be important – at the time, if not still at ours. Historians of philosophy might marvel at how comprehensive the volume is; historians of science at how exclusive.

Granularity and Composition

Analysis of early modern disciplines has proven rewarding, in that it yields problematics fruitfully addressed in recent contributions to the literature, many by authors in this volume. These issues can be distinguished according to three levels of granularity. At the coarsest level, scholars have been concerned with how early modern actors divided themselves up in the first place – what, that is, were the early modern disciplines? This question entails tracing the contours of disciplinary bounds, but also grappling with the very meaning of 'discipline' in the early modern milieu.[19] At a finer level of resolution, scholars have elucidated the activities of the various disciplines that populated the period. This includes analysis of the disciplines' methodologies and core suppositions, as well as description of the problems their participants sought to resolve and the debates about proposed solutions.[20] Finally, there is a group of questions relating to the specific points of contact between disciplines, where representatives of distinct intellectual spheres argue with one another. These "inflection points" garner particular attention since they often generate what appears, to later authors, as radical change – new problems and novel solutions.

These granularities also provide the basic structure of this volume – though, of course, comments on all these interrelated issues can be found throughout. The chapters in the first part engage with questions regarding the early modern disciplines in general. They survey the disciplinary landscape from the beginning to the end of the period, noting especially the

[19] This question of discipline formation is what Richard Rorty calls *Geistesgeschichte* (Rorty 1984). One example is the debate about "natural philosophy" in Grant (1999); Cunningham (2000); Lüthy (2000b). Another is the proposal to use 'chymistry' to distinguish early modern matter theory from later 'chemistry' (Newman and Principe 1998). A recent proposal has been to redefine disciplinary boundaries in terms of unities of practice. See, for example, discussions of the disciplinary unity of early modern "books of secrets" (Eamon 1994; Eamon 2011), the new epistemic genre of "recipes" in medicine and magic (Borrelli 2020), or the formation and evolution of an early modern science of vegetation (Jalobeanu and Matei 2020).

[20] An interesting case is that of the discussions and debates around the early modern set of the disciplines characterized as 'experimental philosophy', or, as Anstey and Jalobeanu call it (ch. 12), 'experimental natural history'. See Harrison (2011); Anstey (2014); Jalobeanu (2015); Feingold (2016).

different ways in which disciplines bounded themselves in relation to each other. The chapters describe how early modern disciplines were organized around the classical texts recovered by the Renaissance (Levitin), and how commentators themselves, like present-day scholars, struggled to characterize and classify the novelty they recognized in their midst (Garber). The essays here also problematize the coherence of "Aristotelianism" (Hattab) and the "Mechanical Philosophy" (Roux, Bellis), usual beginning and ending points in traditional narratives of the Scientific Revolution. Finally, the roles of "external" factors beyond philosophy and science – confessional sects (Blank) and gender and public and private institutions (Detlefsen) – in the formation of disciplines are explored.

The second part of the volume takes up the activities of individual disciplines across the period. The chapters show how these disciplines established themselves upon theoretical problems and how they reached solutions. The essays study the work of early modern practitioners of the "art of thinking" (Corneanu and Vermeir); natural magic (Clucas); the mechanical arts (Klein); learned medicine (Distelzweig and Ragland); the Baconian *scientia* of natural and experimental history (Anstey and Jalobeanu); the "science" of the stars (Omodeo and Regier); mixed-mathematics (Van Dyck); pure mathematics (Guicciardini); and post-Newtonian mathematical physics (Hepburn and Biener). In each case, the authors note developments that significantly impacted the ensuing scientific and philosophical traditions.

Finally, the third part of the volume takes up significant episodes in early modern thought. Of particular interest are instances where disciplinary frameworks faced acute challenges, leading to the formation of new disciplines. These include: Galileo's intrusion of mathematical astronomy into natural philosophy (Miller); the effect of new optical instruments – telescopes and microscopes – on all the empirical sciences (Hamou); the debate about the ontology of the mind arising from Cartesian metaphysics (Pécharman); the reorganization of anatomical science surrounding Harvey's assertion of the circulation of the blood (Manning); the reinterpretation of natural laws following the advent of mathematical physics (Stan); and the important controversies about the ontology of corporeal substances (Gorham and Slowik), God's relation to the natural world (Janiak and Thomas), and which quantity is conserved in physical interactions (Rey).

Altogether, the chapters collected in this volume explore philosophical lessons of the Scientific Revolution, presenting the results of recent work and indicating areas of future interest. Each contribution is "a survey and a step" – a survey of existing literature on the topic and a step toward a novel

understanding. The contributions broaden our understanding of the original constitution of modernity's intellectual traditions. They will be of particular interest to historians of philosophy, science, and intellectual culture, as well as to philosophers of science. Naturally, the subject matter – early modern philosophy and science – is vast, and our coverage cannot be comprehensive.[21] Still, we hope readers find in the book a useful introduction and motivation to read further.

[21] In particular, we regret that early modern alchemy only appears in passing. For recent work, see, e.g., Clericuzio (2000); Hedesan (2016); Rampling (2020).

PART I

*

THE DISCIPLINES

1

The Uses of Ancient Philosophy

DMITRI LEVITIN

Introduction: Restoring Books to the Scientific Revolution

Here is a well-known story. Before sometime in the early modern period, Europeans believed that knowledge of nature came solely from reading books, above all those of Aristotle. Then the humanist rediscovery and translation of various ancient philosophical works led the number of "authorities" to grow, and alongside a monolithic "Aristotelianism" emerged any number of "-isms": Stoicism, Epicureanism, Platonism, Skepticism, and so on. Gradually, philosophers realized that they need not rely on authorities at all, and began to use their own reason, coupled with experience and experiment. Scholasticism and humanism were dead, and the "Age of Reason" had begun, with René Descartes as its iconoclastic father (perhaps with a little help from Francis Bacon).

Caricatured as it is, this narrative still has its adherents, sometimes in totality, more often in parts. But it rests on a fundamental misunderstanding of the pre-modern natural philosophical enterprise stemming from polemics in the period itself. Contrary to the claims of early moderns like Robert Boyle, no Scholastic natural philosopher thought that their task was solely to "interpret Aristotle."[1] However much their enterprise was bookish, their aim was to expound the truth about nature,[2] and the variety of interpretative strategies they used to achieve this goal meant that their approach lived on in the work of some of the most important seventeenth-century natural philosophical innovators.[3] The idea that Descartes marks a transformative point in the history of the human mind, in which bookishness was replaced with abstract reasoning (or text-free empiricism) was the invention of eighteenth-century French propagandists.[4] In reality, even well after 1700, European

[1] Serjeantson (2014), *contra* Gaukroger (2006).
[2] See, e.g., Thijssen (1991).
[3] Dear (1988); Des Chene (1996); Ariew (1999); Edwards (2005); Martin (2011).
[4] Schütt (1998); Edelstein (2010, 24–30).

natural philosophers remained "humanists" in the broad sense of the word: they read and responded to ancient books written in Latin and Greek (and other languages), and "the reading and writing of texts" remained "perhaps the most fundamental activity of all those engaged in the study of the natural world during the period."[5]

All this has significant implications for our understanding of the "Philosophy of the Scientific Revolution" – although what we might mean by that category now also appears much more ambiguous. By recognizing the centrality of continued engagement with ancient textual traditions, we need not deny the occurrence of spectacular changes. However, what we can now do – and so much here remains to be done – is explain how such changes were the product of a distinctly *early modern* mode of doing natural philosophy, rather than of the triumphant emergence of a nebulous scientific or philosophical "modernity."

If we are to understand what early moderns themselves took to be the nature of the transformation occurring in their period, we need to reconstruct their historico-philosophical presuppositions. Ancient texts were not "authorities" to be accepted or rejected. Rather, they were tools for exploring the very nature of what natural philosophy had been, and what it should be. Virtually every methodological discussion in our period was grounded in a genealogy of philosophy's history, in turn usually informed by intimate familiarity with the sources on which that history was based. The most important development was the construction of a historical genealogy in which *any* natural philosophy considered grounded in metaphysics or aprioristic ratiocination came to be seen as an intellectual dead end. This was not quite the separation of "philosophy" from "science" – that would have to wait until the nineteenth century – but it was a major step in that direction.

The Availability and Use of Ancient Sources

Insofar as the "Scientific Revolution" was a self-conscious shift from Scholastic Aristotelianism, we must characterize fairly what was being rejected. Certainly, it was not slavish adherence to Aristotle. Rather, Scholastic Aristotelianism was a form of "metaphysical physics" meant to explain the principles of natural bodies in terms of the "metaphysical parts" said to constitute them – in particular, the seemingly basic ontological entities

[5] Raphael (2017, 6). This is the only sense in which I use 'humanist' – with Copenhaver (2018), I do not believe there was a distinctively "humanist philosophy."

matter and form, the latter reconceptualized in the Latin West as *substantial* form.[6] Since common sense revealed that the world is made up of individuated substances, one had to explain how that could be the case, and what happened when substances changed. The job of the philosopher was to ratiocinate beyond sensible accidents to provide causal answers. And so the focus of pedagogy, commentaries, and reading (at least outside Italy) was primarily on those Aristotelian books – *Metaphysics*, *Physics*, and *De anima* – that could help this kind of "metaphysical-ontological analysis"[7] of material substance, space, time, motion, and so on; and of the problems such analysis produced regarding causality, identity, and the dependence of the world on God. For all its variety, the metaphysical character of this activity remained constant.

The first significant challenge to it came from anti-Scholastic humanists. By the mid-sixteenth century, the humanists had rediscovered, translated, and published the vast majority of the known texts of ancient philosophy, medicine, and mathematics.[8] Information about ancient philosophers and sects could also be gleaned from new publications of ancient anthologies of philosophical *doxa*.[9] Meanwhile, humanist scholars made new anthologies, such as Henri Estienne's *Poesis philosophica* (1573).[10]

As a result, by the seventeenth century, educated men and women encountered ancient philosophy virtually everywhere: in religious apologetics; in scriptural exegesis; and – most important for us – in natural philosophical textbooks.[11] Take just two examples (one from the Catholic world and one from the Protestant): the famous Aristotle commentaries published by the Jesuits of Coimbra between 1591 and 1606, and Marcus Friedrich Wendelin's *Contemplationes physicae* (1625–1628). The former elucidated at length the opinions of the Presocratics chastised by Aristotle in the first book of his *Physics*, categorizing them as materialist animists who philosophized about the first principle in an enigmatic manner akin to that of the Egyptians and Chaldeans.[12] Meanwhile, the Heidelberg-based Wendelin began his

[6] See Maier (1952, esp. 10–27); Maier (1955); Pasnau (2011). 'Metaphysical physics' is from Garber (1992).
[7] Maier (1958, 379).
[8] Kristeller, et al. (1960); Hankins and Palmer (2008). The following summary is expanded in Levitin (2015a, 25–30).
[9] Stanwood (1985); Mansfield and Runia (1997, 1–41); Miller (2007, esp. 107–108); Hankins and Palmer (2008, 66–67); Kraye (2011).
[10] Reverdin (1999); Primavesi (2011).
[11] For the importance of textbooks, see Schmitt (1988). For the humanist turn in Aristotelian natural philosophy, see Kessler (1990).
[12] Conimbricenses (1616, cols. 173–177).

textbook with a detailed excursus into the history of philosophy, in which he offered a long catalogue of those – from the biblical patriarchs to the Chaldeans, Egyptians, and Greeks – who recognized that "the study of nature is clearly a divine and sacred thing," and also intriguingly argued that Aristotle's philosophy had been developed against a skepticism prevalent among pre-Aristotelians, none of whom, he claimed, had considered physics a *scientia*.[13]

Two things are worth noting about these examples, which were read across the continent and could be multiplied exponentially.[14] One is that their authors did not themselves engage in philological scholarship, but drew on humanists who had. Wendelin, for example, derived his "Aristotle versus the skeptics" view of ancient philosophy from Gianfrancesco Pico della Mirandola.[15]

Second, the historical commentary acted as a mechanism for introducing new ideas. It used to be thought that consideration of ancient texts was intrinsically conservative. The reality is quite the opposite. Take, for example, Pierre Gassendi. Gassendi is often said to have adopted as much of "Epicureanism" as was compatible with Christian theology. But Gassendi approached ancient texts not as authorities, but as part of a historical-philosophical exercise designed to explain which avenues in philosophy had proved useful and which were dead ends.[16] He disagreed with many essential Epicurean tenets, and dogmatically grounded his natural philosophy in sense experience and experiment.[17] Gassendi's comparative analysis then functioned as the basis for a full system of natural philosophy in his posthumous *Syntagma philosophicum* (1658).[18]

Another important (but virtually unknown) example can be found in David Gregory's lectures on astronomy, delivered in mid-1690s Oxford. The statutes of his Savilian Chair compelled Gregory to lecture on Ptolemy's *Almagest*. Gregory complied by following the subjects raised in that work, but incorporated whatever he wanted into the content, using the lectures – as Gassendi had used the *Syntagma* – to summarize ancient and

[13] Wendelin (1648, sigs. A3e–A5r, 2b–14a).
[14] For how a student read Wendelin, see Serjeantson (2016, 70–71).
[15] Levitin (2015a, 302). For Pico (not to be confused with his uncle Giovanni), see Schmitt (1967).
[16] Joy (1987, esp. 83–105); Palmerino (1999, 277).
[17] See, e.g., Matton (1994); Massignat (2000); Palmerino (2004); Fisher (2005, 19–190); Sakamoto (2009).
[18] See LoLordo (2006).

modern answers to astronomical problems, with the Newtonian usually coming out best.[19]

Rewriting the History of the Study of Nature

How did these historical-genealogical preoccupations feed into natural philosophical discussion? In the sixteenth and seventeenth centuries, the question of natural philosophical method was more debated than perhaps at any other time in European history.[20] In particular, the role of observation, experience, and experiment was continuously discussed, since what sometimes came to be known as "experimental philosophy" seemed not to fit the traditional (Aristotelian) definition of natural philosophy as a speculative (or contemplative) science of causes.[21] This methodological discussion was usually conducted in a historical mode. Four particularly important developments can be identified. In each case, we can draw a direct line from the humanist-inspired challenge to Scholasticism to the methodological discourse of the great names of seventeenth-century natural philosophy.

Humanist Anti-Aristotelianism

Historians of philosophy sometimes have a tendency to dismiss humanist anti-Aristotelian writings as superficial and unimportant.[22] But whatever the humanists' logical inconsistencies, there is ample evidence of the long-term influence of humanist anti-Scholasticism, and then anti-Aristotelianism, not only on the first generations of *novatores*, but even on the self-declared "experimental philosophers" of the late seventeenth century.

The primary aim of the great humanist anti-Scholastics – most important were Lorenzo Valla, Gianfrancesco Pico della Mirandola, Mario Nizolio, and Petrus Ramus – was not to erect a new natural philosophy, but to heighten the institutional prestige of the *studia humanitatis* by reforming (or even eradicating) dialectics, and by promoting rhetoric and a purified Latin style.

In pursuit of this aim, they developed three arguments that proved particularly important. The first was an argument for linguistic simplification. Thus, for Valla "the principal task he has . . . imposed on himself is to cut back this useless superstructure of technical jargon and empty concepts" – the ten categories, six transcendental terms, the predicables, form and matter, act and potency – "by reducing them to what he considers the basic elements of

[19] Aberdeen University Library, MS 2206/3/3.
[20] See Gilbert (1960); Dear (1988); Di Liscia, et al. (1997).
[21] See Dear (2005, esp. 393–397); Levitin (2019).
[22] See Nauta (2009, 3–4, 296, n. 12).

a common-sense worldview. These basic elements are things we perceive either physically or mentally and that may be analysed as qualified substances."[23] The anti-abstractionism of such talk made it appealing to later natural philosophers, whose aim was not to secure an institutional place for Ciceronian Latin, but to replace Scholastic Aristotelianism.[24]

A second, concomitant element was a reductionist (or simply destructive) attitude toward metaphysical parts. This was often accompanied by the portrayal of metaphysics as a non-discipline, which, it was claimed, was the product of a mistaken reification of purely mental entities that had fatally corrupted correct philosophical method. Such a reification was unjustified by any sensory evidence, and, according to the humanists, served to "form a theoretical superstructure that . . . prevents a clear view of the world of concrete things."[25] Valla reduced the ten categories to three – substance, quality, and action. Nizolio accepted only substances and qualities. Pico, inspired by the newly rediscovered works of Sextus Empiricus, questioned the ability of the Aristotelians – or of anyone – to grasp the essences of things on the basis of the study of accidents, and thus to build a *scientia* at all.[26] This line of critique remained well known, partially culminating in Francisco Sanches's *Quod nihil scitur* (1581) and Gassendi's *Exercitationes paradoxicae adversus Aristoteleos* (1624).[27]

Such critiques certainly overplayed the extent to which the Scholastics claimed to possess knowledge of essences or even substances.[28] Nonetheless, they implied a quite radical disciplinary reconfiguration; specifically, the rhetorical opposition between "metaphysical" and "non-metaphysical" forms of natural philosophy (and theology). So, for example, Nizolio explicitly contrasted a metaphysical mode of "pseudophilosophy" that dealt with abstractions of the mind – which he associated with Plato and the Aristotle of contemporary philosophers – with one that examined individual objects perceptible to the senses, which he associated with the Aristotle of the animal books.[29] Such language soon offered ammunition for natural philosophers in their disciplinary battles.

The third element of the anti-Scholastic critique most relevant to natural philosophy was the deployment of the latest philological techniques, especially in Greek, to argue that the *Corpus Aristotelicum* was horribly corrupt and

[23] Nauta (2009, 13). [24] E.g., Locke, *Essay*, III.vi.24.
[25] Nauta (2012, 38). It remains to be seen how indebted this move was to Ockham or any other tradition of "nominalism" – the best recent scholarship suggests not much (Nauta 2003; Nauta 2012, 48–49).
[26] Pico della Mirandola (1520, cxxxii^{r}–cxxxvii^{v}); Schmitt (1967, esp. 110–118).
[27] Gassendi (1649); Sanches (1988, 215–216). [28] Hattab (2009, 74–75); Pasnau (2011, 635).
[29] Nizolio (1670, 256, 258–261).

confused. Particularly important was the claim that the metaphysical and logical components had become confused with the natural philosophical ones. We find this theme developed above all by Pico and later in the sixteenth century by Francesco Patrizi.[30] This is hardly a philosophical argument, but it proved very important, especially when combined with the historical point that Aristotle had misrepresented his predecessors when he criticized them, and plagiarized them whenever he offered anything of value. This point was still being faithfully repeated by Royal Society apologists a century later.[31]

Aristotelian Naturalism and the Emancipation of Natural Philosophy from Metaphysics

The arguments of the humanist anti-Aristotelians would have had little impact had they not been taken up by natural philosophers. But they were adopted by some prominent Italians, often called "secular" Aristotelians and above all based at the University of Padua. In these figures, we find the first emancipation of institutional natural philosophy from metaphysics, and a concomitant emphasis on the role of sensory experience. Under the influence of Averroës and rediscovered Greek commentators, especially Alexander of Aphrodisias, the lay natural philosophers primarily taught not the subject matter of the *Physics* and *Metaphysics*, but that of *De generatione et corruptione* and Book IV of the *Meteorology*; namely, sensible bodies and their principles – the four elements and the sensible qualities.[32] In the teaching and writing of professors such as Pietro Pomponazzi and Simone Porzio, we witness both a strong emphasis on sense perception as the source of natural philosophical knowledge, and a polemical insistence – clearly derived from the humanist anti-Aristotelians – that this emphasis was incompatible with Scholastic natural philosophy.[33] None of this made them "Empiricists." But when Porzio, for example, argued that the Scholastics' "metaphysical" approach led them to bastardize true Aristotelianism with the concept of substantial form, and that the nature of things was determined by their sensible qualities (and was hence open to empirical investigation), he articulated a conclusion that was still being promoted in seventeenth-century textbooks.[34]

[30] Reiner (1955); Wilmott (1984, 286–288); Deitz (1997); Deitz (2007); Deitz (2019). For Bacon's later adaptation, see Bacon (1996–, 4:86).
[31] Levitin (2015a, 303). [32] Lohr (1988); Kessler (1995); Kessler (2001); Kessler (2011).
[33] Kessler (2001, 85); Bakker (2007, 168, n. 39); De Angelis (2010, 78–84).
[34] Porzio (1553, II.1, f. 85). See Del Soldato (2010, 61–100, 173–195); Kessler (2011, 90–95). For the seventeenth century, see, e.g., Pemble (1629, 30–32); Meldrum (1659, 14); White (1663, 109–110); Heereboord (1680, 162–186).

Gradually, this move inspired a widespread shift in the teaching of Aristotelian natural philosophy, with far more attention devoted to the "naturalistic" parts, and even a conscious division between those and the "metaphysical" parts of physics.[35] It also precipitated the development and prevalence of the kind of anti-speculative language usually more associated with the later seventeenth century. For example, Bernardino Telesio – who would prove so important to Francis Bacon – began his great treatise, *De rerum natura* (1565), by promising that, unlike his predecessors, he would "not deal with metaphysical principles of nature, which are the result of mere theoretical speculations," but that his philosophy of nature will be "based on what is obvious to sense-perception or can be derived immediately from it."[36] Here we find adumbrations of the idea that the study of nature should not seek fundamental explanations, but rather content itself with the knowledge of properties, accidents, and intermediate causes. Inevitably, Telesio presented this as a return to the "true" method of philosophy.

The neo-humanist claim that the Scholastics had corrupted natural philosophy by mixing it with metaphysics and abstract ratiocination continued to be a battle-cry of "new" philosophers in the seventeenth century; often, we can find them taking it directly from one of their sixteenth-century predecessors, especially Patrizi. It can be seen in figures as varied as Bacon, Niccolò Cabeo, Kenelm Digby, Gassendi, Thomas Hobbes, and Isaac Newton.[37] It was also redeployed by apologists for the Royal Society, such as Joseph Glanvill.[38] And we find it prominently stated in the methodological writings of Boyle, who complained again and again that philosophy had become "rather metaphysical, or logical, than grounded upon the principles and *Phænomena* of Nature."[39] This was not a function of Boyle being a representative of an English "Empiricism" or "experimental philosophy"; rather, it was the culmination of a century and a half of pan-European methodological debate.

[35] Blum (1988).

[36] Telesio (1586, 1); Kessler (2001, 97–99). See Martin (1998, 179–183). Some anti-Aristotelians thought Telesio had not escaped metaphysics enough; see Trabucco (2019, 111–113).

[37] Digby (1644, 344); Cabeo (1646, 4:418); Gassendi (1649, 126, 154–155); Hobbes (1655, 20–22); Gassendi (1658a, 1:133–134); Kusukawa (1996, 56–58); Bacon (1996–, 4:86); Leijenhorst (2002, 17–55); Martin (2011, 109); Hobbes (2012, 3:1078–1082); Levitin (2015a, 242–252); Levitin (2016).

[38] Glanvill (1665, 71).

[39] Boyle (1999b, 343; also 289, 294, 309, 344, 472, 479). Also Royal Society, Boyle Papers 8, f. 184^{r}; Boyle (1999c, 163).

Humanist Medicine, Experience, and Natural History

A third key development was the uptake of such anti-metaphysical historical genealogies by physicians and natural philosophers with medical interests.[40] Physic was the primary profession in which one might pursue an interest in nature, and the *persona* of the physician was inevitably tied to the discipline's history; specifically, to the methodologies adopted by ancient sects known primarily from the works of Galen and Celsus's *De medicina*: the Rationalists (or Dogmatics), the Empirics, and the Methodics. This ancient sectarianism raised the stakes in the professional rivalries of the period. Paracelsian physicians, apothecaries, and surgeons were identified with the Empirics, sometimes even by themselves,[41] and thereby denigrated by learned (i.e., university-trained) physicians, who saw the Empirics' lack of philosophical acumen as both intellectually shallow and dangerously irresponsible.

In turn, the practitioners sought to turn the identification into a positive by elevating the status of experiential knowledge. They did so through recourse to the many new texts humanists had made available, above all the Hippocratic corpus and Dioscorides. Paracelsians, such as Petrus Severinus, Gerhard Dorn, Joseph Duchesne, and Jan Baptist Van Helmont,[42] always insisted on the importance of knowledge acquired via the senses, telling a story of the corruption of such knowledge from a Hippocratic high point to the destructive abstractions introduced by Plato and Aristotle and then by "speculative" Galenism,[43] thereby asserting that experiential Hippocratism was the foundation of the *true* "learned" tradition.[44] Indeed, it was within "neo-Hippocratic medical circles with the recovery of the ancient Empiric/Skeptic philosophical vocabulary" that we first witness burgeoning talk of *observatio* and the introduction of '*phaenomena*' to refer to objects being observed.[45]

The learned physicians responded by developing historical narratives to present *their* tradition as the truly experiential and experimental one. They argued that Rationalist physic had been more experiential than medieval and Arabic physicians had recognized.[46] It helped that the publication of the

[40] Despite some pioneering studies, the importance of medicine to the early modern intellectual transformation is underappreciated. For the medicine–natural philosophy relationship, see Schmitt (1985); Mikkeli (1992, 135–147); Maclean (2002, 70–76). For medicine and the "new philosophy," see Cook (2011); Siraisi (2012); Ragland (2017).

[41] See Maclean (2002, 78).

[42] Pumfrey (1998).

[43] Halleux (1980); Pagel (1982b, 58–59); Webster (1982, 52); Joly (1995).

[44] Severinus (1571, 2, 73–74); Shackelford (2002, esp. 143–208).

[45] Pomata (2011a, 65); Pomata (2011b).

[46] See Siraisi (1987, 66, 70, 73–75); Siraisi (1997, 27–28); Maclean (2002, 20–21, 339–340). For changing perceptions of the Arabic philosophical inheritance, see Hasse (2016).

Greek Galen (1525) had revealed the importance of anatomy to Galenic physic. Andreas Vesalius's famous preface to his *De humani corporis fabrica* (1543) was only one manifestation of these developments.[47] In his influential commentary on Galen, Giovanni Argenterio – probably influenced by the anti-Scholastic writings of Pico and others – claimed that the status of medicine had been historically degraded by "idle philosophers, so as to affect for themselves a greater status." He particularly railed against the imperious claims of metaphysics to provide all arts and sciences with their principles.[48]

In combination with these negative claims about the corrosive effects of speculative philosophizing on medicine, both learned physicians and their competitors also argued positively that ancient physicians had practiced many "practical" medical disciplines, and that far from being split into specialist roles, such as the physician and apothecary, the ancient model had been that of the total *medicus*.[49] The result was the postulation of a "Rational Empiricism" – a focus on the empirical component of learned medicine, including case histories, with an insistence that this was never reducible to *nuda experimenta*, but was methodically organized and rationally interpreted.[50] Emphasis on observation thus effected a simultaneous "philosophization" of experiential medicine and "empiricization" of natural philosophy, accompanied and motivated by historical revisionism. Hieronymus Fabricius ab Aquapendente's Padua-based "Aristotle project," meant to ground all of medicine in anatomy, directly informed William Harvey's defense of the philosophical status of his discovery of the circulation of the blood, predicated on the claim that "observation itself was a type of knowledge."[51]

Since the early Royal Society was largely a circle of post-Harveian philosophers and physicians,[52] justifications of observation and experiment via medical history proved very influential in later seventeenth-century England. But the same pattern can be seen across the continent, where physicians emphasized the importance of Harvey's discovery, and experimental method more generally, by evoking Hippocrates (or even Galen) as

[47] O'Malley (1964, 317–318). For the subsequent "Galen project," see Cunningham (1997, 88–142).
[48] Argenterio (1550, 9–10); Argenterio (1592, 1:42). See Siraisi (1990, 172–175).
[49] Findlen (1994, 246); Levitin (2015b).
[50] Lonie (1985); Nutton (1988, 123–124); Nutton (1989); Maclean (2002, 201–204); Pomata (2005, 136–137); Pomata (2010); Pomata (2011a, 58–59); Maclean (2021).
[51] French (1994, 316). For Fabricius, see Cunningham (1985); Mikkeli (1992, 148–159); Cunningham (1997, 167–190). For Harvey, see Webster (1967a); Whitteridge (1981); Wear (1983); Schmitt (1984b); French (1994); Sgarbi (2013, 180–184). See also Manning in this volume (ch. 20).
[52] Frank (1976).

a proto-experimentalist.[53] Significantly, such arguments rejected the dangerous reliance on bare experience promoted by the Empirics, and insisted that the foundations of the ancients' successful therapeutic and pathological methods – especially semiotic and indications – had to be preserved.

The use of historical argument to raise the status of experiential knowledge and deride non-sensible speculation was also evident in less mainstream traditions. Chymists and iatrochymists attempted to align their discipline with the spirit of Presocratic philosophy. Most important here was the *persona* of Democritus the dissecting naturalist, derived from two unconnected pseudepigrapha: the *Epistola ad Damagetum*, in which Democritus appears as a comparative anatomist, and the *Physica et mystica*, in which he appears as an inheritor of Egyptian alchemy. These allowed early modern practitioners, such as Duchesne and Andreas Libavius, to present themselves as inheritors of a Democritus who followed "an experimental approach . . . that involved microscopical anatomy, chemical analysis, and mechanistic model-building."[54] They argued that the Aristotelians' errors in doctrine – above all hylomorphism – were the result of a mistaken method; namely, the application of logic and metaphysics to natural philosophy, a hubristic rationalism derived from Plato and shared by the Galenists. The "Democritus" they posited against such a method was not a reductive atomist, but a chymical experimentalist who stood with those who (in Libavius's words), "when reporting on corporeal nature and erecting a science of nature, see that the fictions of our mind should not be followed, but only those things should be posited which the senses, with reason and experience, show to be in the nature of bodies."[55]

It was at the hands of physicians that the methodological attack on Scholastic–Aristotelian physics first came together with an admiration for the supposedly experimental Presocratics, whose matter theory was cautiously adopted without being co-opted into a reductionist ontology. In turn, physicians inspired the wider dissemination of such claims, especially in France. Perhaps most famous are the virulently anti-hylomorphic theses proposed by Étienne De Clave, a physician and iatrochymist undoubtedly influenced by Severinus and Libavius, which shocked Paris in August 1624, as well as the *Philosophiae naturalis adversus Aristotelem libri XII* (1621) by the medically trained Sébastien Basson, which reserved its greatest praise for the

[53] See Preiser (1969); Lindeboom (1974, 145a); Lonie (1981); Lo Presti (2010); Ragland (2012, 9–10); Levitin (2015a, 255–260, 271–295, 309–312); Maclean (2021).
[54] Lüthy (2000a, 470). Also Newman (2006, 73).
[55] Libavius (1607, 713). See also Quercetanus (i.e., Duchesne) (1603, 4–5); Joly (1995).

mythical Hippocrates, student of Democritus.[56] And it was from medical and chymical writers such as Severinus, in combination with the anti-Aristotelian tradition of Patrizi, that Francis Bacon obtained his view of the history of natural philosophy, in which the earliest Greek philosophers – above all Democritus and Hippocrates – "directed themselves to the search for truth more quietly, seriously, and simply . . . with less affectation and ostentation" than their sophistic followers, above all Aristotle and Galen.[57] Variants of this historical vision remained influential into the eighteenth century.

Mathematization

A fourth forum in which historical arguments were crucial for methodological developments was the so-called "mathematization of nature." Contrary to a long-established story, this had little to do with a reified "Platonism" or "Pythagoreanism." Rather, ancient mathematical texts inspired both new research projects and novel conceptions of what natural philosophy could be (and had been). This was above all the case with Pappus, Archimedes, Hero of Alexandria, and the pseudo-Aristotelian *Quaestiones mechanicae*, the use of which led to an increased integration of mathematics into university curricula.[58]

These texts were again not used as authorities, but stimulated research projects. For example, the project of explaining all simple machines in terms of the lever, associated with Pappus, was revived by Guidobaldo del Monte, directly inspired Galileo, and "in looser and broader fashion, became a key component of seventeenth-century mechanics."[59] Meanwhile, François Viète presented his pioneering work on algebra as a restoration of the ancient art of geometrical analysis, again as (supposedly) described by Pappus. Other mathematicians developed reconstructions of lost ancient texts mentioned by Pappus (such as Apollonius's *On the Determinate Section*), using the latest scholarly techniques, including collation of manuscripts and conjectural emendation. In Willebord Snell's case, these had been learned from the great humanist scholar Joseph Scaliger.[60] The impact of all these ancient texts was also enhanced by their incorporation into the schemes of pedagogical reformers, above all Ramus.[61] Indeed, the reconstruction of the "lost"

[56] Basson (1621, sigs. [¶¶4]^{r}–[¶¶7]^{v}), where Hippocrates is treated as the high-point of pre-Aristotelian knowledge of nature. See also Nielsen (1988); Lüthy (1997).
[57] Bacon (2004, 114). See Smith (1979, 18); Whitaker (1990); Gaukroger (2001, 104–118); Rossi (2013); Levitin (2015a, 237–240); Maclean (2021).
[58] Boas (1949); Rose and Drake (1971); Valleriani (2009); Laird (2017).
[59] Bertoloni Meli (2006, 16). See further Rose (1975, 280–291); Bertoloni Meli (2006, 19–65).
[60] De Wreede (2007, esp. 53–64). [61] Goulding (2010, 19–74).

or "hidden" analysis of the Greeks remained the aim even of Descartes in the *Géométrie* (1637).[62]

The ideas contained in the recovered texts fed into the disciplinary battle about mathematical explanations – specifically, whether they offered *causal* explanations typical of natural philosophy. The mathematical texts presented new and different criteria for what constituted a legitimate explanation. For example, Archimedes' *Equilibrium of Planes* introduced the concept of "center of gravity" (notoriously left undefined), which was intrinsically difficult to redescribe in any causal terms.[63] Similarly, the same work, as well as several of the problems in the *Quaestiones mechanicae* – which most people accepted as being by Aristotle himself – proposed reduction to the lever as a model of explanation, a fact that was exploited by those both inside and outside the universities to suggest the potential for mechanics to explain natural phenomena.[64]

These alternative modes of explanation were used to reconfigure natural philosophy, and – again – to critique traditional approaches. Already in the mid-sixteenth century, Giovanni Battista Benedetti, well known for his proto-Galilean account of free fall, contrasted Aristotelian philosophy with what he called an Archimedean "mathematical philosophy."[65] In a similar if less aggressive vein, the great Urbino historian of mathematics Bernardino Baldi wrote in his *Vita di Archemede* (c. 1590) that "mechanics being a physical subject but nevertheless demonstrable by mathematical reasoning, it seems that Aristotle, leaving aside the mathematical aspects, preferred to draw his demonstrations from physical principles," with the mathematical component that completed the theories only supplied by Archimedes.[66] Modern natural philosophers were to combine the two.

Most importantly, the mathematicians adopted the humanist critique of "metaphysical physics." Natural philosophers critical of the mathematicians' pretensions to philosophical status, such as Alessandro Piccolomini and Benito Pereira, had argued that mathematics was a non-causal discipline because its demonstrations did not include essences. In response, some mixed mathematicians started to celebrate this fact.[67] Drawing on the anti-essentialism of the humanist anti-Scholastics, they noted that because mathematics did not deal with essences or natures – which, as every natural

[62] See Cifoletti (1996); Kaplan (2018).
[63] For an attempt to do so by Guidobaldo dal Monte, see Van Dyck (2006).
[64] Bertoloni Meli (1992); Laird (2000, 32–36); Bertoloni Meli (2006, 24–25); Palmieri (2008).
[65] Benedetti (1969, 196). See Bordiga (1985). [66] Drake and Drabkin (1969, 14).
[67] Dear (1995, 162–163).

philosopher admitted, were obscure and difficult to grasp – it was able to establish certainty about nature within its own sphere. Exactly this argument appears in Federico Commandino's dedication in his seminal edition of Archimedes' *Opera* (1558), referring to the history of philosophy and its inability to establish consensus about fundamental principles:

> As for the principles of all things, from which everything originates, when might we find three or at most four philosophers who thought the same thing? For on motion, the void, time, the very elements and their nature, the various and mutually conflicting opinions of the philosophers easily show that physics [*physiologia*] is founded on various conjectures rather than on the firmest proofs; the best we can do is apprehend what is most probable in it.[68]

Accordingly, Commandino went on, of the three speculative sciences – physics, metaphysics, and mathematics – the first two were only probable, whereas the last offered certainty, and thus ranked highest, a conclusion that extended to its application to natural philosophy and medicine.[69]

The argument that mathematics offered certainty about properties while philosophers squabbled about essences would be central to the campaign to mathematize natural philosophy all the way to Newton (and beyond). The most important development of this position appeared in Giuseppe Biancani's *De mathematicarum natura* (1615).[70] Biancani reveled in the "reproach" leveled at mathematics: "the ignobility of its subject [i.e. quantity], namely, that it is an accident." "It is better," he replied,

> to get to know innumerable, marvelous truths about an accident, than always to be cast from one side to the other, by the whirlpool of a thousand opinions and dissensions, especially concerning material substance, and hence never to arrive at the cognition of any substance at all.[71]

Moreover, because mathematics produces certain – and even causal – knowledge of properties (*affectiones*) known to the senses, it followed that mixed mathematics produced the most certain type of knowledge about natural things, in line with Aristotle's own dictum that "the 'what' is to be known by those who perceive, but the 'why' by the mathematicians, for they have the demonstrations of the causes."[72] For instance, the reduction of a wedge to the principle of the lever

[68] Commandino (1558, sig. *2r).
[69] Similar arguments are offered by Jacopo Mazzoni. See Mazzoni (1597, 159–164, 187–197); Purnell (1972, 282–292); De Pace (1993, ch. 4).
[70] Galluzzi (1973, 56–65); Giacobbe (1976); Wallace (1984, 141–148); Dear (1995, 36–41); Mancosu (1996, 16–19).
[71] Biancani (1996, 202).
[72] Biancani (1996, 206), somewhat misrepresenting *Posterior Analytics* 79a3–6.

provided a *causal* explanation of its operation and power. And Archimedes' explanations of the centers of gravity of the classical solids, recently completed by Luca Valerio in his *De centro gravitatis solidorum* (1605), were, according to Biancani, proof that "this powerful [mathematical] philosophy manifests the structure and symmetry of the whole world."[73]

It only remained for those less conciliatory than the Jesuit Biancani to take the next steps: (i) to condemn more violently the philosophical search for causal explanations grounded in the knowledge of essences; and (ii) to affirm with more conviction that the principles of a mathematical science of properties were derived from sense experience, but nonetheless amenable to mathematical study that produced something akin to demonstrative, certain knowledge. One person who did both was Biancani's acquaintance Galileo. In a debate about buoyancy, some natural philosophers accused Galileo of abandoning true philosophical method with his mathematical explanations. Those philosophers did not claim that Aristotle was an infallible authority, freely acknowledging that his conclusions could be improved upon. But, they argued, all Galileo was saying was that "lightness is the cause of floating," whereas Aristotle had actually offered an *explanation* for the natural cause of lightness.[74] Galileo responded by reformulating causation in phenomenological terms, partly by condemning the search for internal, structural explanations, such as those grounded in the four elements, in favor of explanations in terms of proximate causes known immediately to the senses, such as weight.[75] By the time of the Third Letter on Sunspots (1612) such an argument had blossomed into a full, anti-essentialist historical vision and concomitant research program. On the one hand there were those philosophers who had vainly sought "to penetrate the true internal essence of natural substances"; on the other were those like him, who "content ourselves with a knowledge of some of their properties [*affezioni*]."[76]

Galileo's most important followers – Bonaventura Cavalieri, Evangelista Torricelli, Benedetto Castelli, and Vincenzo Viviani – proceeded in this "Archimedean" vein, happily applying the lessons of mathematics and mechanics to the physics of motion without making any foundational ontological or causal claims (this was what so annoyed Descartes about Galileo's own work).[77]

[73] Biancani (1996, 206–207). On Valerio, see Baldini and Napolitani (1991); Napolitani and Saito (2004).

[74] Anonymous (1890, 165). See Drake (1970a); Shea (1972, 14–48); Galluzzi (1979).

[75] *Discorso intorno alle cose che stanno in sù l'acqua, o che in quella si muovono* (1612) (Galilei 1890–1909, 4:86–87).

[76] Galilei (1890–1909, 5:187); Drake (1957, 123).

[77] Descartes (1964–1974, 1:392, 2:380); Shea (1978); Ariew (1986).

But they now justified this approach with much grander genealogies of the history of "true" philosophical method. For example, Baliani would launch a long attack on philosophical explanations of first principles, the two archetypal ones being Democritean atomism and Aristotelian hylomorphism. According to him, they imagined something to be true and then proceeded to posit effects that necessarily followed, a type of explanation no better than the imaginary epicycles of the ancient astronomers. Far better were the approaches of the mathematicians and the chymists, who established their principles only through experience and experiment, a rather remarkable conjunction of the mathematical and iatrochymical strands of anti-philosophical historical thinking.[78]

By this point, such an approach had spread well beyond Italy, above all to France, where Marin Mersenne was pivotal in promoting the idea that natural knowledge was to be had only of appearances, treated probabilistically as a mathematical science of accidents.[79] Already in the 1640s, this ideal closely reflected the reality of actual natural philosophical practice and publication, for across Italy and France – and even in the dreary English hinterland – the years 1630–1650 witnessed an extraordinary range of mixed mathematical publications, often now couched in the language of "physico-mathematics."[80]

This creeping colonization of natural philosophy by mixed mathematics was frequently re-described in historical terms as a conflict between experiential and rationalist modes of philosophizing. We find this, for example, in Pierre Guiffart's 1647 defense of Blaise Pascal's experimental proof of the vacuum, which tells a strikingly simple story about the history of natural philosophy:

> Although Monsieur Pascal's experiments [*experiences*] seem new to us, it seems that they were performed in the past, and that many ancients have taken them as grounds for maintaining that there may be a void in nature, or even that the void is a principle of nature [at this point the marginal note names Democritus, Leucippus, Diodorus, Epicurus, and Lucretius]. But those who have since asserted the contrary have been content to follow their own reasoning, which, not being founded on the certitude of the senses, now finds itself overthrown by their testimony.[81]

[78] Baliani (1666, 39–58, esp. 43–44).

[79] See, e.g., Mersenne (1625, 206–218); Mersenne (1634, 18); Mersenne (1932–1988, 8:582–583). See also Dear (1984, 190); Dear (1988, 68–69); Buccolini (2013). I differ from the characterization of Mersenne as a "resolute Aristotelian" in Garber (2004, 152). See, e.g., Mersenne (1634, 9).

[80] See Dear (1995, 168–169); Bertoloni Meli (2006, 161–162).

[81] Guiffart (1647, 54).

Guiffart was no atomist – his evocation of the Greeks is merely a device to argue that the *a priori* reasoning supposedly being used by Pascal's critics had no place in natural philosophy. There is something similar in this history to the physicians' and alchemists' story of an experiential Democritean–Hippocratic naturalism being corrupted by Aristotelian–Galenic speculation.

The most elaborate historical defense of the scientific status of mathematics, and its applicability to natural philosophy, appeared in Isaac Barrow's Lucasian lectures of the 1660s. Barrow, who was familiar with most of the continental literature, including Biancani, defended the status of mathematics as a causal *scientia* and its centrality to natural philosophy on the basis that all natural phenomena are caused by matter and its motion in space.[82] To this end, he completely rewrote the history of both disciplines to tell a story of the corruption of a "total" mathematics by its separation into "pure" and "mixed" variants, like Biancani focusing on Aristotle.[83]

Conclusion: Anti-Philosophy as the Philosophy of the Scientific Revolution

A continuing infatuation with a putative canon of "great philosophers" – Descartes, Hobbes, Baruch Spinoza, John Locke, and Gottfried Wilhelm Leibniz – as well as the reification of a non-existent "English empiricism," has obscured the fact that the institutionalization of the new science in the second half of the seventeenth century in the Royal Society, the Académie des Sciences, and the Accademia del Cimento was almost entirely the work of university-trained physicians and mixed mathematicians, often working in collaboration.[84] This can be revealed by a simple prosopographic analysis, or by examining those societies' activities. For example, the men who were most active in the early Royal Society – John Wilkins, Thomas Henshaw, William Croone, Jonathan Goddard, Walter Charleton, Christopher Merrett, and John Wallis – had almost all received their intellectual formation in mid-century Oxford, and their interests were almost entirely confined to post-Harveian physiology or pure and mixed mathematics.[85]

It comes as little surprise, then, that the most important historical justifications of the new natural philosophy followed directly in the footsteps of the

[82] Kargon (1966); Mahoney (1990b); Shapiro (1993, 31–36); Mancosu (1996, 20–24); Malet (1997); Stewart (2000); Dunlop (2012).
[83] Barrow (1685); Barrow (1734). See Levitin (2015a, 316–322).
[84] Bertoloni Meli (2008).
[85] Frank (1976). For the Académie, see Sturdy (1995, 16–21).

humanist anti-Aristotelians, the physicians, and the mixed mathematicians. The famous and oft-cited defenses of the Royal Society by Thomas Sprat and Joseph Glanvill consisted largely of historical arguments drawn from Patrizi, Gassendi, and others, to the effect that abstract speculation, aprioristic ratiocination about first principles, and metaphysics had drawn philosophy away from the true path of experience and experiment – Democritus (implausibly) remained a representative of the latter.[86] But as should now be clear, there was nothing uniquely English about such claims.

Notably, such historico-philosophical assumptions made many mixed mathematicians – in France as elsewhere – immediately skeptical of Descartes's project to build a new "metaphysical physics." For instance, Mersenne's friend Gilles Personne de Roberval, the Ramus professor of mathematics at the Collège Royal, thought Descartes only the latest of the "dogmatists of our time."[87] Similarly, Jean Le Clerc's popular physics textbook[88] taught students to oppose the approach taken by Descartes and his followers, which he presented as an aprioristic deductivism no better than Scholasticism. All in all, Le Clerc concluded, rather than hypothesize about ultimate principles, "it is more useful to be ingenious men who, like Democritus, wear away life in experiments and the search for truth, so as to light the way for others."[89] This is of course a reference to Democritus the experimentalist and friend of Hippocrates, rather than the atomist.

This may come as a surprise to those used to the story in which the heart of the Scientific Revolution is the shift from hylomorphic to mechanistic metaphysics. But that story – like so many other narratives of European modernity – gives far too much importance to Descartes. Descartes and his followers were undoubtedly ontological mechanists, and often came up with elaborate metaphysical schemes to defend such a mechanism. But the Cartesians were outliers. To most, ontological mechanism appeared ridiculous, especially when it came to explaining organic life.[90] For most of the new natural philosophers, Descartes was repeating the great error of the past: metaphysical speculation about first principles. This is why he was frequently compared to Epicurus – it is not that anyone thought that Descartes was an atomist; rather, they thought that both Descartes and Epicurus had built overconvenient systems that were not grounded in experience, and could

[86] Levitin (2015a, 295–313). [87] Malcolm (2002, 168–171). [88] Le Clerc (1696).
[89] Le Clerc (1696, sig. 10⋆v, 12⋆r–v, 491). [90] Des Chene (2001).

not explain it. As Boyle put it, both the Cartesians and Epicureans erred in "pretend[ing] to explicate every particular Phænomenon by deducing it from the Mechanicall affections of Atomes."[91]

This is not to say that the natural philosophers turned to some kind of animism.[92] Rather, they disdained *all* speculation concerning underlying principles and ontology. For example, Pierre Petit, an important experimentalist, astronomer, and engineer, dismissed *both* Aristotelian and atomist explanations of heat and cold as pointless "metaphysical" speculations. His "mechanism," like that of so many others, was confined to second causes – hence he was known as "that Archimedes of our time."[93] We find something similar in Boyle, who, despite his dreams of building a full mechanical philosophy where everything was explicable only by matter in motion, disavowed the aim of writing "a whole Systeme, or so much as the Elements of Natural Philosophy."[94] Such a disavowal, expressed in historical terms and combined with a willingness to limit oneself to phenomenological explanations of specific phenomena in terms of secondary causes, lay at the heart of anything that could be called the Scientific Revolution.

On these terms, "the philosophy of the Scientific Revolution" was an *anti*-philosophy: a historico-philosophical worldview – first developed by humanists in the sixteenth century – in which the search for metaphysical or ontological first principles was condemned as the greatest source of philosophical error and fruitlessness. The importance of this worldview – the specific manifestations of which could take many forms – is confirmed by the fact it was also held by Newton, who, in the 1710s, developed a hyper-elaborate history of philosophy and religion, in which an original philosophical religion that recognized heliocentrism was gradually corrupted by the (non-)discipline that was metaphysics, generating everything he despised: idolatry in pagan religion, a speculative natural philosophy, and the doctrine of a consubstantial Trinity in Christian theology.[95] A mathematical physics that posited universal gravitation without ever explaining its operation at the fundamental ontological level allowed one to explain many phenomena without ever having to engage in the kind of

[91] Royal Society, Boyle Papers 7, f. 166[r]. [92] See Levitin (2015a, ch. 5); Levitin (2018).
[93] Petit (1671, xxiii, 49–50); Le Gallois (1672, 42). See De Waard (1925); Gabbey (1984, esp. 101–102).
[94] Boyle (1999a, 148).
[95] See Bodmer Foundation, MS Bodmer; National Library of Israel, MS Yahuda 15 (also at www.newtonproject.ox.ac.uk/catalogue/record/THEM00218). For a full account, see Levitin (forthcoming).

speculative philosophizing that had led not only to philosophical error, but also to religious corruption. Newton's singular mathematical genius is not in question. But, despite all the idiosyncrasies of his vision, he was still a product of a shift two centuries in the making.[96]

[96] Many thanks to Jennifer Rampling, Dana Jalobeanu, and David Marshall Miller for comments on earlier versions.

2

Novatores

DANIEL GARBER

In early 1655, a book appeared in Paris: *De la perfection de l'homme*, a miscellany on topics ranging from the utility of knowledge to the proper way of instructing princes. One essay in the collection offers a "Summary of the strangest opinions of the modern innovators [*Novateurs Modernes*] in philosophy."[1] The author, Charles Sorel, was popular in France,[2] and his "Summary" was meant to inform the general (educated) public of a group of "moderns" who had proposed new ideas. Sorel offers a long list of such authors, including Bernardino Telesio, Francesco Patrizi, Girolamo Cardano, Petrus Ramus, Galileo Galilei, Johannes Kepler, Giordano Bruno, David Gorlaeus, Sébastian Basson, Tommaso Campanella, René Descartes, and many others. These moderns are treated as a unified category, called the '*novatores*'. This term was already in wide circulation in the seventeenth century; highly polemical and philosophically charged, it was found at center-stage in many early modern debates.

This chapter traces the emergence and the complex posterity of this term. I argue that the term transcends the familiar divide between actors' and analysts' categories. '*Novatores*' is a *polemical* category; and tracing its origins, history, and use gives us precious insights into the dynamics of the great transformation of philosophy usually designated by another polemical label – the Scientific Revolution.

[1] *"Le sommaire des opinions les plus estranges des Novateurs Modernes en la Philosophie"* (Sorel 1655, 209–275). The essay is reprinted with small changes in Sorel (1668, 360–449). Sorel's examination of the *novateurs* began earlier. Sorel (1644) has an appendix, paginated separately from the book, titled "Table of ancient philosophy," of which chapter six is "Of those who wrote against Aristotle" (Sorel 1644, 76–112). While this begins with some ancient opponents of Aristotle, the bulk of the essay is about moderns. On some aspects of Sorel's treatment of the *novateurs*, see Del Prete (2001a).

[2] Sorel was a libertine and an author of popular romances who, later in life, turned toward history. His encyclopedic project *La science universelle* was published in four volumes between 1635 and 1644. For general background on Sorel, see Roy (1891); Verdier (1984). On *La science universelle*, see Picardi (2007).

Novelty, For and Against: Philosophy and Religion

'*Novatores*' was a term with wide cultural resonances. In religious matters, innovating was, in turns, feared and criticized. Both Protestants and Catholics attempted to rewrite history in order to present themselves as restoring ancient truths, rather than inventing new doctrines. Meanwhile, both camps insisted that the other was guilty of introducing new beliefs and novel heresies. '*Novator*' was thus a pejorative used by theologians to vilify those with whom they disagreed.[3] However, the close connection between theology and Aristotelian philosophy had a long tradition in the European universities, which the Reformation did little to shake. Conversely, this alliance also raised the stakes of philosophical novelty, since giving up Aristotelianism could threaten theology.[4] Consequently, the epithet migrated from theology into philosophy to designate one who rejected Aristotelianism. By the 1620s, '*novator*' was used to condemn those who had given up Aristotelian natural philosophy and were trying to replace it with something different.[5]

In his 1623 commentary on Genesis, *Quaestiones celeberrimae in Genesim*, Marin Mersenne gives a list of moderns who criticized Catholic followers of Aristotle, like himself, and who advanced views in contradiction with the Aristotelian philosophy taught in the schools. He wrote:

> Hence I only want to warn you, dearest reader, that I have followed the order of commentaries so that I might refute the atheists, magicians, deists, and things tending to that sort, which now have started to run wild through the whole world; and that many questions, which otherwise might seem curious, have been taken up so they may show the followers of Campanella, Bruno, Telesio, Kepler, Galileo, [William] Gilbert, and other recent philosophers that what they say is false, namely that the Catholic doctors and theologians follow Aristotle alone, and swear by his words, even if experience and the phenomena are contrary.[6]

Mersenne refers to this group as *novatores*.[7] Later in the work, he adds the names of Francis Bacon, Robert Fludd, Basson, and Nicholas Hill.[8] He gives

[3] See, e.g., "*Novateur*" in Furetière (1690, vol. 2).
[4] On the introduction of Aristotle into European universities, see Marrone (2009); Martin (2014, ch. 1). See Schmitt (1983); Kusukawa (1995); Martin (2014, chs. 3–5) on Aristotle in the Renaissance university and in the Reformation and Counter-Reformation. For a survey of recent work on Protestant Scholasticism, see Heyd (2016, 463–466).
[5] See Garber (2002a).
[6] Mersenne (1623, Preface, p. č).
[7] Mersenne (1623, Preface, p. č ij).
[8] Mersenne (1623, col. 1838).

a similar list in his next book, *L'impiété des deists, athées et libertins de ce temps* . . . (1624), including this time Giulio Cesare Vanini, Nathanael Carpenter, Gorleus, Basson, Hill, Campanella, Bruno, Cardano, Pierre Charron, "and some others."[9]

Mersenne was by no means alone in grouping these thinkers together. Gabriel Naudé, a French writer often included among the so-called *libertins érudites* of the first half of the seventeenth century, mentions the *novateurs* in philosophy, as well.[10] In his *Apologie pour tous les grands personnages qui ont esté faussement soupçonnez de magie* (1625), he wrote, citing a list of thinkers that overlaps considerably with Mersenne's:

> All of the truths about nature that he [i.e., Aristotle] says he knew are today rendered very suspect and doubtful by a swarm of innovators [*novateurs*] that increases from one day to the next under the leadership of Telesio, Patrizi, Campanella, Bacon, Giordano Bruno, and Basson, who in reality have no other aim but to give the elbow to this philosophy . . . .[11]

A similar list – and similar intent – can also be found in the writings of Jean Cécile Frey, who taught at the University of Paris from 1607 to 1631.[12]

It was not only in Paris that philosophers, both supporters and opponents of Aristotle, recognized the emergence of a group of thinkers attempting to articulate alternatives to the dominant Aristotelianism. Descartes refers to the "*novatores*,"[13] as do his enemies in Utrecht. In his anti-Cartesian polemic, *Admiranda methodus* (1643),[14] Martin Schoock discusses Descartes squarely in the context of the *novatores*, among which he is counted.[15] Gorleus and Nicolaus Taurellus – very much at issue in the earlier disputes in Utrecht – come up often in the book,[16] but Schoock also includes Ramus, "the two Patrizis" (i.e., da Cherso and da Siena), Telesio, and Basson.[17] Throughout,

[9] Mersenne (1624, 236–240). See also Mersenne (1625, 109).
[10] On Naudé, see Bianchi (1996); Schino (2014).
[11] Naudé (1625, 331). He gives a similar list in Naudé (1627, 135), where they are also identified as the "innovators" (*novateurs*).
[12] See Frey (1646, 29), translated in Frey (2003). On Frey, see Blair (1993); Blair (1994) and the introduction to Frey (2003).
[13] See, e.g., Descartes (1984–1991, 3:26–27); Descartes (1996, 1:158).
[14] See Schoock (1643); French translation in Verbeek (1988, 153–320).
[15] For an account of the dispute in Utrecht over Descartes's philosophy, in addition to the copious commentary in Verbeek (1988), see the excellent account in Verbeek (1992, ch. 2).
[16] On the alleged connection between Descartes, Gorlaeus, and Taurellus, see Lüthy (2019).
[17] Schoock (1643, 6); Verbeek (1988, 183). See also Schoock (1643, *6, 42); Verbeek (1988, 165, 200). For other mentions of *novatores* in texts related to the controversy, see Verbeek (1988, 94, 98, 100, 101, 103, 109, 112, 113, 144).

Schoock criticizes innovation and innovators, especially Descartes, in the most extravagant and derisive terms.

The *novatores* are treated more favorably in the writings of Adriaan Heereboord, who divides them into two camps: those who are merely destructive of the old and those who may contribute to a new project. The former includes Juan Luis Vives, Ramus, Patrizi (da Cherso, in his *Discussiones peripateticae*), Gorlaeus, Campanella, Telesio, Basson, and the "*fratres Bootii*" (Gerard and Arnold Boate). In the latter group he includes Patrizi (in his *Nova philosophia*), Bacon, John Amos Comenius, and Descartes. According to Heereboord, Descartes's published writings "presented the true key to the true philosophy, disclosed not a few of the mysteries of nature, and opened the door for entering into the fixed truths about things."[18] Later in the same volume, in an essay defending the freedom of philosophizing (*libertas philosophandi*), Heereboord cites Ramus, Carpenter, Telesio, Patrizi, Basson, Campanella, and Fludd.[19]

Thus, by the time Sorel published his essay on the *novateurs*, the category was well established in learned circles, as was a more-or-less standard list of figures included in it. But that list has a curious feature: it is largely limited to authors from the mid- and late sixteenth century onward. Yet Aristotelian philosophy had been controversial from the time it first came into the Latin West in the twelfth and early thirteenth centuries, and remained controversial throughout the Middle Ages and Renaissance. There were numerous rejections of Aristotelian philosophy before the figures that Sorel and his contemporaries chose to focus on.[20] Why earlier thinkers were ignored is not entirely clear. It is possible that much of the earlier history of the rejection of Aristotle was simply not known until later. For example, while there were a series of important condemnations of Aristotle in Paris in the thirteenth century, culminating in that of 1277, it seems very little was known about them through most of the seventeenth century.[21] It is also possible that the later Renaissance critics of Aristotle had simply disappeared from memory, and were no longer read or discussed.[22] But whatever the reason, the

[18] Heereboord (1654). Pagination is complicated in this volume – the passage discussed is on the first page 28. See also Dibon (1990, 204–205). On Heereboord, see Ruestow (1973, 44–66); Verbeek (1992, 35–40); Hotson (2020, 125–144).

[19] Heereboord (1654, 334). In this later essay Heereboord does not appeal to the distinction between those anti-Aristotelians who simply destroy the old and those that build the new.

[20] For a survey of the rejections of Aristotelian philosophy in the thirteenth century and later, see Martin (2014, 22–26, ch. 2).

[21] Ariew (2011, 250–257).

[22] Perhaps significantly, in the appendix to Sorel (1644) mentioned above, when discussing the opponents of Aristotle, Sorel goes directly from St. Justin Martyr to Patrizi and later figures (Sorel 1644, 76–112).

novatores were not as new as they (or their critics) may have thought that they were.

In general, the *novatores* were very controversial – as we have seen, novelty did not have a good name.[23] Some of the hostility derived from the general view that those who advocate new opinions do so not out of an interest in the truth, but only to call attention to themselves. In his 1655 essay, Sorel dismissed those "who follow immediately all new opinions, and who blindly follow them only to distinguish themselves from others."[24] In the early eighteenth century, Georg Saur, Jesuit professor at the Würzburg, criticized the *novatores*, noting:

> We are very often deceived by new things, since we are delighted by them, even if they have not yet been proved true by the touchstone of truth. It is one thing to seek after new things, and quite another to seek after truths . . . .[25]

But I suspect the connection between Aristotelianism and theological orthodoxy drove much of the hostility toward the *novatores*, since the latter was often associated with civil unrest. The wars of religion were very much on the mind of many of those who opposed the *novatores*. At the end of the sixteenth century, Montaigne wrote:

> it takes a lot of self-love and presumption to have such esteem for one's own opinions that to establish them one must overthrow the public peace and introduce so many inevitable evils and such a horrible corruption of morals . . . .[26]

Montaigne had theology in mind, but for the later century, innovation in natural philosophy was of a piece. In a 1624 pamphlet, Jean-Baptiste Morin noted "almost all heresies have departed from the philosophy of Aristotle." But what is particularly dangerous is that these heresies lead to social chaos: "Then these heresies have divided minds, from which have followed the division and ruin of provinces and entire kingdoms."[27]

In his *Recherche de la vérité* (1674–1675), Nicolas Malebranche wrote:

> truth and novelty cannot be found together in things of the faith. Because men do not wish to make the distinction between truths that depend upon

[23] Interestingly, there was not nearly the same opposition to new technological inventions; see Keller (1972). Not unrelated was the wide interest in new lands, new peoples, and new experiences. This thirst for empirical novelty is closely connected with curiosity, which, as several authors have noted, underwent a major transformation in the early modern period, from a vice to something generally shared and approved of. See Daston (1995b); Daston (1995a); Harrison (2001).

[24] Sorel (1655, 273). [25] *"Ad amicum lectorem"* in Saur (1708).

[26] Montaigne (1962, 1:127); Montaigne (1965, 87). [27] Morin (1624, 4–5).

> reason and those that depend upon tradition, they do not consider that one should learn them in completely different ways. They confuse novelty with error, and antiquity with truth. Luther, Calvin, and others innovated, and erred; therefore, Galileo, Harvey, Descartes are mistaken in anything novel they say. Luther's impanation is novel, and it is false; therefore Harvey's theory of the circulation of the blood is false, since it is new. It is also for this reason that they call both heretics and the new philosophers indifferently by the odious name of innovator.[28]

As Malebranche suggested, the opprobrium about novelty in theology and religion came to taint innovation in philosophy, as well, though he himself approved of novelty in natural philosophy. Malebranche, of course, was writing from the Catholic perspective, and while Protestants may have disagreed about which religious doctrines and practices were novel, they agreed that novelty in religion is not good. And many on both sides of the confessional divide thought that novelty in natural philosophy is not good, either.

Who Were the *Novatores*?

The list of *novatores* evolved over the years, as new names were added, but its beginnings remained reasonably constant. Telesio was almost always among the earliest, followed by Francesco Patrizi da Cherso and Campanella – that is, late-sixteenth-century Italians.[29] Bacon, in fact, considered Telesio "the first of the new philosophers [*novorum hominum primus*],"[30] and, in this way, the true father of modern philosophy. Sorel endorsed this perspective, proposing that Telesio deserves credit for "having had the confidence to strike the first blow" against Aristotle,[31] and giving credit more generally to the Italians for their contributions.[32] Sorel focuses on Telesio's main philosophical work, *De rerum natura, iuxta propria principia*, and gives a brief account of seven of the nine books of the expanded edition of 1586.[33] He emphasizes Telesio's aggressive opposition to Aristotle and his attempt to

[28] *Récherche de la vérité* 2.2.3 (Malebranche 1979, 1:213; Malebranche 1997, 139). Blaise Pascal offers a similar sentiment (Pascal 1963, 230–232).

[29] On Sorel's treatment of Italy in the essay, see Del Prete (2001a).

[30] *De principiis et originibus* (Bacon 1996–, 6:258–259). Interestingly, even when Bacon wrote this evaluation sometime in the 1610s, Telesio's reputation was somewhat in eclipse. He adds that Telesio's thought is "a philosophy which is not of course very famous nor widely accepted." On the dating of the text, see Bacon (1996–, 6:xxviii–xxxi).

[31] Sorel (1655, 221). On Telesio's reception in the seventeenth century, see Garber (2016a).

[32] Sorel (1655, 215).

[33] Versions of the book in two volumes had been published in 1565 and 1570. For general accounts of Telesio's thought, see De Franco (1995); Mulsow (1998); Bondi (2018). For shorter accounts see Leijenhorst (2010); Boenke (2018). See also Omodeo (2019).

provide an explicit alternative to Aristotelian natural philosophy, grounded in the opposition between the hot and the cold.[34]

Sorel next takes up Patrizi da Cherso, focusing on his *Nova de universis philosophia* (1591),[35] and presenting him as a kind of follower of Telesio[36] who also sought to revive Platonic doctrines. To Telesio's principles of hot, cold, and corporeal mass, Patrizi added space, light, warmth, and fluidity.[37] Sorel finishes his account of what he considers the first opponents of Aristotle with another Italian, Cardano, and a Frenchman, Ramus. But there are two others normally included among the earlier Italians, Campanella and Bruno, whom Sorel treats later in his essay.

Campanella was a strong defender of Telesio's philosophy, which he discussed in his early *Philosophia sensibus demonstrata* (1591). He was also a defender of Galileo, which Sorel mentions in connection with his discussion of Galileo's condemnation.[38] But in the portion of his essay devoted to Campanella, Sorel chooses to emphasize his breadth of interests and his bold style.[39] His work is filled with matters "quite distant from the opinions of the Peripaticians."

> His physics is completely filled with new and renewed opinions, and one can be sure that no author ancient or modern has proposed something which he has not touched in some way in this place, and joined to it some additions or reflections. If in his other works he did not take the liberty of distancing himself from received opinions, he has at least accompanied them with some boldness.[40]

But in his account, Sorel only explicitly mentions one of his works, *De sensu rerum et magia* (1620).

While Sorel's account of Campanella lasts only two pages, he gives an extensive and detailed account of Bruno, emphasizing the diversity and richness of his philosophy and giving a detailed account of Bruno's doctrine

[34] Sorel (1655, 215–218).
[35] Sorel (1655, 219–223). For general overviews of Patrizi, see Crespi (1931); Vasoli (1989); Leinkauf (2010).
[36] Sorel (1655, 222).
[37] Sorel (1655, 221). It is not clear how warmth (*la chaleur*) is different from heat (*le chaud*). Sorel thinks that Patrizi is not altogether fair to Telesio here, insofar as he included "light" as part of "heat" (Sorel 1655, 222).
[38] Sorel (1655, 236). See Campanella (1622); Campanella (1994).
[39] Campanella was a prolific writer, with works in natural philosophy, metaphysics, ethics, theology, politics, poetry, astrology, magic, as well as a celebrated utopia, *La cittá del sole* (Campanella 1981). For full-length accounts of Campanella's thought, see Headley (1997); Ponzio (2001); Ernst (2010); for a shorter account, see Ernst (2014).
[40] Sorel (1655, 251).

of the infinity of the cosmos and the plurality of worlds.[41] It is noteworthy that, while Sorel considers the scientific objections to Bruno's view, he does not consider the obvious theological problems raised by other earths, especially concerning the creation and salvation of their inhabitants. Indeed, Sorel ends his discussion of Bruno with this comment on his having been burned at the stake for his views: "It is unfortunate that a man who had written such very beautiful things would perish in so unfortunate a way."[42]

Among the next generation of *novatores*, Sorel discusses Gorlaeus (or van Goorle), Carpenter, and Basson, all of whom had works published in 1620 or 1621. Though present on virtually every list of *novatores* in the period, these later *novatores* get shorter treatments relative to the earlier Italians.

In his brief discussion of Gorlaeus, Sorel notes the *Exercitationes philosophicae* (1620), but not his *Idea physica* (1651). Interestingly, he does not mention Gorlaeus's atomism, a feature that most impresses many modern readers, but is impressed by Gorlaeus's opposition to Aristotelian theoretical philosophy and his assertion that there are just two elements, earth and water.[43] Carpenter is pictured as a lover of empty paradoxes who claimed that place is nothing, that all the elements are heavy, that senses cannot err, that comets are not fiery meteors, and other things "against the common opinion."[44] Sorel seems particularly vexed by Carpenter's assertion that fire is moist (*humide*). Though he admits that there is some moisture in fire, "one should not call it moist on account of the very small amount of moistness it has."[45]

[41] Sorel (1655, 239). Bruno's corpus is vast, and the literature on him is equally extensive. For general accounts, see Rowland (2008); Blum (2012a). For an account of Bruno's scientific works, see Gatti (1999). For a shorter overview, see Knox (2019). Sorel discusses Bruno's *De innumerabilibus, immenso et infigurabili, seu de universo et mundis libri octo*. See Bruno (1591) or Bruno (1879–1891, vol. 1, parts 1–2). There is no published translation of the work in English, but there is a French translation (Bruno 2006). There is also a recent collection of articles that together constitute a close commentary on the text (Granada and Tessicini 2020).

[42] Sorel (1655).

[43] Sorel (1655, 248). On Gorlaeus, see especially Hattab (2009, ch. 7); Lüthy (2012). Gorlaeus – or, at least, the editor of his posthumously published *Exercitationes* – considered his philosophy anti-Aristotelian; the subtitle tells us that in the book, "a great many principal doctrines of the peripatetics are overturned." This view of Gorlaeus was certainly shared by Sorel and many others. However, Helen Hattab has shown convincingly that in a number of respects, Gorlaeus remained an Aristotelian. See Hattab (2016) and her chapter in this volume. But as we shall see below, this paradoxically does not necessarily undermine his status as a *novator*.

[44] Sorel (1655, 248). Sorel's discussion is based on *Philosophia libera* (1621). During his lifetime, Carpenter was probably better known as a preacher than as a natural philosopher. Relatively little has been written about Carpenter. For his life, see Benedict (2004); for his scientific works, see Jones (1961, 65–71); Tyacke (1978, 79–82, 89).

[45] Sorel (1655, 249).

Basson was widely known for his *Philosophiae naturalis adversus Aristotelem libri XII* (1621), a book probably more cited than read. One suspects that this may have been true of Sorel, who very quickly and superficially notes Basson's "curious thoughts" concerning the nature of things, matter, form, mixtures, motion, and the action of primary qualities. Sorel emphasizes Basson's anti-Aristotelianism, but not the atomism noticed by many other readers.[46]

Galileo and Descartes also get significant treatment. In a collective section on astronomical *novatores* that includes brief discussions of Nicolaus Copernicus, Kepler, and some other lesser-known figures, Sorel emphasizes Galileo's astronomical work and discusses at some length both his telescopic observations and his defense of Copernicanism. He notes that Galileo was condemned and had to abjure, but remarks that, even so, "his advocates were scarcely frightened by this, and in no way hold that it is sinful to believe that the Sun is immobile and that it is the Earth which turns."[47]

The treatment of Descartes is among the longest in Sorel's essay.[48] Sorel begins with the *Discours de la méthode*, summarizing the main themes, including the *cogito* and the appeal to God, before giving brief indications of the three essays that follow, the *Géométrie*, the *Dioptrique*, and the *Météores*. But in the remainder of the essay, the focus is squarely on Descartes's scientific works. Sorel skips over the *Meditations* and goes directly to the *Principia*, giving brief accounts of the cosmology and terrestrial physics contained therein, particularly the denial of the vacuum. The last work mentioned is the *Passions de l'âme*, though Sorel does not detail its contents. Sorel admits that there are those who think that Descartes had borrowed from others, in particular, Democritus, Bruno, and Jean d'Espagnet,[49] but he is quite positively impressed by Descartes's philosophy: "Among all the innovators one sees none who have gone so far from the commonly accepted thought."[50] Sorel also notes that Descartes "is not at all lacking for

[46] Sorel (1655, 250–251). Sorel devotes only one short paragraph to Basson, who was an enigmatic character. For a thorough and entertaining account of Basson's life and thought, see Lüthy (1997); for a short summary, Lüthy (2008). On Basson's atomism, see Gregory (1964); Nielsen (1988).

[47] Sorel (1655, 237).

[48] Sorel (1655, 252–258).

[49] Sorel (1655, 256). D'Espagnet was a French chymist and the author of the anonymously published *Enchiridion physicae restitutae* (1623). It came out in a French translation by Jean Bachou, under the title *Philosophie naturelle restablie en sa pureté* (1651). In his introduction, Bachou gives a history of the *novatores* of his day, including many of the names seen in other sources, and ending with fulsome praise of Descartes, before revealing the name of the author of the anonymous treatise that he had translated. D'Espagnet is discussed in Sorel (1655, 249–250).

[50] Sorel (1655, 256).

followers."[51] The account of Descartes is even longer and more complimentary in the emended version of the essay that Sorel published in 1668.[52]

Many of the *novatores* mentioned in other lists are missing from Sorel's essay. The most notable omission is perhaps Francis Bacon – an author widely read at the time, and whose *De augmentis scientiarum* is discussed by Sorel later in the same volume in an essay on the encyclopedists.[53] Also missing are Gilbert and Kenelm Digby. While Pierre Gassendi is mentioned just barely, it is not for his project for reviving Epicurean atomism, but for his astronomical writings.[54] But it is also interesting to note some of the figures that Sorel treats who are largely ignored in other lists. The sixteenth-century Huguenot naturalist and potter Bernard Palissy – otherwise virtually unrepresented in lists of *novatores* – gets one of the longest treatments, almost as long as Descartes's.[55] Sorel also includes a long section on the *"novateurs chymistes,"* which includes short discussions of a number of figures that are generally not listed among the *novatores*.

While there is much more to be said about the individual members of this group, this should give a brief sketch of one perspective on the wide variety of characters that make up the *novatores* as they were conceived.

Ancients, Moderns, and Making the Old New

While novelty and innovation seem to be at the center of the dispute over the *novatores*, it is important to distinguish this from another, superficially similar dispute in the seventeenth century, the so-called *Querelle des anciens et des modernes*. While the debate over the relative value of the ancients and the moderns goes back much earlier, what is called the *Querelle* proper began in 1687 with the publication of Charles Perrault's poem, *Le siècle de Louis le Grand*, where Perrault defends the value of the literature of his age over that of the ancients.[56] The debate soon expanded to cover culture more generally, including philosophy and the sciences.[57] Unlike in the *Querelle*, the issue with the *novatores* was largely limited to natural philosophy. Furthermore, any contention between the ancient world and the moderns was marginal, at best. For the *novatores* and their opponents, novelty simply meant rejecting the authority of Aristotle; the fact that Aristotle was an ancient was largely

[51] Sorel (1655, 258). [52] Sorel (1668, 418–426). [53] See Sorel (1655, 309–312).
[54] Sorel (1655, 234). [55] Sorel (1655, 243–248). On Palissy, see Biswas and Biswas (2008).
[56] The literature on the *Querelle* is vast, but the essays in Lecoq, et al. (2001) are a good introduction.
[57] See, e.g., Wotton (1694); Baker (1699).

irrelevant. Furthermore, while some of the *novatores* presented views of the world that they and their contemporaries took to be genuinely new, for many *novatores* the innovations proposed had, at the very least, a resonance with past philosophies. Indeed, in a number of important cases, the "new" philosophies proposed were explicit revivals of ancient thinkers. Thus, to be a *novator* was to depart from Aristotle, either by inventing something new, or by reviving something old – or by doing both at the same time.

In the case of at least some of the *novatores*, there is a studied ambiguity between doing something new and reviving something old. This is the case with Francis Bacon. While Bacon is quite happy to flaunt the term "new" in his titles, as in the *Novum organum* or the *New Atlantis*, and to emphasize his break with Aristotle and current philosophical fashions, he also expressed the view that Aristotle (and Plato) had self-consciously destroyed the views of those who had gone before them in order to elevate their own views:

> the more ancient of the Greeks, Empedocles, Anaxagoras, Leucippus, Democritus, Parmenides, Heraclitus, Xenophanes, Philolaus, and the rest . . . did not (so far as we know) open schools but go on with their pursuit of truth more quietly, rigorously and simply, i.e., with less affectation and showing off. Thus I judge them to have done the job better, though their works were, with the passage of time, wiped out by those light-minded people whose works were more satisfying and pleasing to the capacity and attitudes of the vulgar herd [i.e., Plato and especially Aristotle] – time (like a river) carrying down to us matter lighter and full of wind, while letting the heavier and solid stuff sink.[58]

In this way, Bacon sometimes represents himself as attempting to revive an earlier and more substantial philosophy. This is the conceit of his *Sapientia ueterum* (1609), where he claims to show the philosophy hidden in ancient Greek mythology.[59] Similarly, the principal meaning of the Latin term '*instauratio*' is "to restore or repair," suggesting that his "Great Instauration" (*Instauratio magna*) is a renovation of earlier views. It is not clear how far to press this reading, though, since, as we have seen, there was pressure on those presenting new ideas to hide behind the authority of antiquity.

In a similar move, in his *Aristarchi Samii de mundi systemate . . . libellus* (1644), Gilles Personne de Roberval used the prestige of ancient wisdom to

[58] *Novum organum* I.71 (Bacon 1996–, 11:114–115). Elsewhere, Bacon is blunter still: "Aristotle, it must be confessed, busied himself, like an Ottoman Turk, in the slaughter of his brethren, and with success" (Bacon 1857–1874, 3:602; Farrington 1964, 84).

[59] Bacon (1857–1874, 6:617–686). See Manzo (1996).

support Copernicanism. The work is presented as if it were a newly discovered Arabic manuscript by Aristarchus, transcribed and translated by Roberval, with his commentary.[60] It is not clear, though, that anyone really believed that the text was a genuine ancient text, or that Roberval had intended it to be understood as a genuine revival of the historical Aristarchus.[61]

Bacon and Roberval may not have been entirely serious in their appeal to the ancients. But there were others of their contemporaries who did use such strategies in earnest. Gassendi, often numbered among the *novatores*, spent years editing and translating the works of Epicurus and explicitly advocated a modernized and Christianized version of Epicurianism in his writings.[62] For Gassendi, the new philosophy was a revival of ancient thought.

Some called this way of doing something new by reviving something old *"philosophia novantiqua."* In a pair of lectures that the Leipzig professor Jakob Thomasius gave in 1665,[63] he distinguished two classes of *novatores*: the *libertini* and the *novantiqui*.

> I might observe that *there are two varieties of those who tear down the lovers of the peripatetic doctrine*. For one kind [i.e., the *libertini*] denounce this whole kind of servitude, as they call it, and have introduced the freest means of philosophizing into the schools. The other kind [i.e., the *novantiqui*] do not damn servitude, as much as they do the master, and they deny that there is good reason either for the ancients to have elected or for us to have retained, from among all the others, Aristotle, the worst master of knowledge in comparison to the remaining ancients. . . . [T]he one calls the mind to unbridled freedom, while the other revives the domains of ancient princes long since banished into exile. Though they may be unlike one another, indeed, they are not so unlike that they cannot be acknowledged as brothers.[64]

Though it may not be apparent on the face of it, Thomasius claims that the *novantiqui*, like the *libertini*, are just *novatores*:

> These [*novantiqui*] are a bit wiser than those libertines, who have introduced complete anarchy. For they retain the form of the control of a monarch, for

[60] Roberval (1644, p. ă ij).
[61] The identification of Copernicus with Aristarchus was used by opponents of heliocentrism, as well. In 1631, Libertus Fromondus published an attack on Copernicanism under the title *Ant-Aristarchus* (Fromondus 1631), which portrayed Copernicus's heliocentrism as a mere revival of Aristarchus.
[62] This is a central theme of Joy (1987).
[63] The lectures were later published in Thomasius (1693, 437–451, 465–480). For an excellent account of Thomasius, see the introduction to Leibniz and Thomasius (1993).
[64] Thomasius (1693, 438–439).

> which nothing can be healthier for men's *ingenium* in the Republic of Letters. But if you empty out what is hidden in their purses, you will discover that they [i.e., the *novantiqui*] are led not so much by an eagerness for the truth, as by a lust for novelty. . . . Doubtless their deceit is hidden. They look for ancient names, lest they be shunned as obvious *novatores*.[65]

One kind of *novantiquus* innovated through the words of an ancient other than Aristotle. But there was another kind, as well: some *novantiqui* attempted to show how one of the *novatores* was really consistent with Aristotle, properly understood. This strategy was particularly popular among some later followers of broadly Cartesian natural philosophies. It is followed by René Le Bossu in his *Parallele des principes de la physique d'Aristote et de celle de René Des Cartes* (1674),[66] where Aristotle and Descartes (and his disciple Jacques Rohault) get a full exposition, often side by side. In the penultimate chapter, Le Bossu presents "Aristotle and M. des Cartes opposed,"[67] a series of brief summary paragraphs, Aristotle in Roman type and Descartes in italics, that show their contrasting positions. But then, in the final chapter, "Aristotle and M. des Cartes in accord," he shows how the two can be reconciled. In a subtle (perhaps *too* subtle) discussion, Le Bossu goes back and forth between Descartes and Aristotle, on questions of philosophical method, doubt, authority, and trust in the senses. He concludes:

> And thus since these two ways of treating physics are necessary, and are only two different parts of one and the same body, we do not at all reject the help that we can derive from them, and that they reciprocally give to one another.[68]

Somehow, in his grand reconciliation of Aristotle and Descartes, Le Bossu forgets to mention the incompatibility between Aristotle's hylomorphism and Descartes's mechanism. But no matter – in the grander scheme of things, they are, to his mind, consistent.[69]

The point of comparisons like Le Bossu's is to show the compatibility of the *novatores* with Aristotelian philosophy. But there is a more direct way of doing it, yet another kind of *philosophia novantiqua*. Here the strategy is not merely to compare Aristotle with the moderns, like Descartes, but to

[65] Thomasius (1693, 469). [66] On Le Bossu, see Schmaltz (2017a, 94–95).
[67] Le Bossu (1674, 276). [68] Le Bossu (1674, 319).
[69] One can find a similar project in Jacques Du Roure's *La physique expliquée suivant le sentiment des anciens et nouveaux philosophes; et principalement de Descartes* (1653) and *La philosophie divisée en toutes ses parties . . . tirez des anciens, et des nouveaux auteurs; et principalement des Péripatéticiens, et de Descartes* (1654). See Ariew (2014); Schmaltz (2017a, 90–92); Roux (2020).

interpret Aristotle as saying exactly the same thing as the moderns. This is the strategy followed in Johannes de Raei's *Clavis philosophiae naturalis, seu introductio ad naturae contemplationem Aristotelico-Cartesiana* (1654).[70] It is obvious from the title that de Raei's interest is reading Aristotle through Descartes. There are complexities in de Raei's reading – he is very aware that there is much in Aristotle that is quite inconsistent with Descartes – but he emphasizes what he sees as correspondences between Aristotelian doctrines and mechanist principles. (Descartes himself does not actually come up very much in the body of de Raei's text.) But de Raei was absolutely clear in proposing that Aristotle himself could only be understood as having advanced the same doctrine as Descartes: that matter is extended stuff, which is subject to form, that is, shape.[71] And with this, Descartes and Aristotle are united.[72]

Fifteen years later, de Raei's *Clavis* was discovered by a young student at Leipzig, Gottfried Wilhelm Leibniz. In an enthusiastic letter in October 1668, the young Leibniz told his teacher, Thomasius, whose lectures about *novantiqui* and *novatores* he probably attended, about the grand new vision he found in the *Philosophical Transactions of the Royal Society*, which he had just discovered.[73] Furthermore, Leibniz told his *magister*, such a reformed philosophy will in no way be inconsistent with the philosophy of Aristotle:

> De Raei well shows, in his *Clavis philosophiae naturalis*, that the obscurities of Aristotle are the effects of the scholastic smokescreen, and that Aristotle himself is astonishingly in conformity with Galileo, Bacon, Gassendi, Descartes, and Digby.[74]

The rest of the relatively short letter is filled with illustrations of how key terms and principles in Aristotelian philosophy can be understood in very broadly mechanist terms, including form, matter, motion, and space. Thomasius, himself committed to a kind of academic Aristotelianism, was not impressed. But this did not prevent the very young Leibniz from forging ahead in his next letter, the famous letter of 20/30 April 1669,[75] where he developed the ideas from his earlier letter at greater length. As in the earlier

[70] See Verbeek (1992, 71–73); Strazzoni (2011); Schmaltz (2017a, 77–80).
[71] Raei (1654, 53).
[72] One can find a similar strategy in Holwarda (1651). Though he does not explicitly mention Descartes, his account of moderns (*neoterici*) is very close to Descartes's physics.
[73] Leibniz (1923–, 2-1:17–19).
[74] Leibniz (1923–, 2-1:18). Bodéüs (1991), though, argues that de Raei was less important than Thomasius himself in influencing Leibniz's project for a "reformed philosophy."
[75] Leibniz (1923–, 2-1:23–38); Leibniz (1969, 93–104). Leibniz considered this letter important enough as a statement of his views at the time to publish the whole letter (with some

letter, the young Leibniz goes on to show how Aristotle, when we reject the perversions introduced by the Scholastics, can be properly interpreted as fully consistent with the moderns. He argues that Aristotelian matter is just extension and impenetrability, and Aristotelian form just figure, and that all change for Aristotle can be reduced to local motion, just as in the moderns. And then, having reconciled Aristotle with the *"philosophia reformata,"*[76] Leibniz turns to show that this fusion of Aristotle and the *novatores* is true.

This particular variety of the *philosophia novantiqua* project reveals something very interesting about the seventeenth-century debate between the Aristotelians and the *novatores*. Those *novatores* who attempt to reconcile a modern philosophy like Cartesianism with Aristotelianism argue that Aristotle, properly interpreted, turns out to be consistent with their favorite strand of modern philosophy. The two are reconciled by arguing that Aristotle has been incorrectly interpreted by the schoolmen, and that we must reject Scholastic philosophy for a proper interpretation of Aristotle's thought. Even so, the *novantiqui* remain *novatores*.

But there is an interesting parallel among the Aristotelians. Recent scholars have shown the way in which some early modern Aristotelians were open to new ideas. Some were interested in the new philosophies and empirical observations and attempted to deal with the problems that these new ideas raised for the Aristotelian philosophy.[77] But though they were open to considering the new, they remained Aristotelians. There certainly were *novatores* openly hostile to Aristotelianism of any sort, and Aristotelians openly hostile to the *novatores* and any challenge to orthodox Aristotelianism. But there also seems to have been a gray area, thinkers who clearly identified as either Aristotelians or *novatores*, but yet were, in a way, not entirely unsympathetic to the other side. For this group of thinkers, it may not be easy to tell on which side they belong by virtue of doctrine alone. As with political parties, intellectual commitments are only one factor in determining the group to which you belong. To take the case of

relatively small changes) in the introduction to his 1670 edition of Marius Nizolius, *De veris principiis et vera ratione philosophandi libri IV* (Leibniz 1923–, 6-2:433–444).

[76] Leibniz (1923–, 2-1:33); Leibniz (1969, 100). On Leibniz and the so-called "reformed philosophy," see Mercer (1990).

[77] Schmitt (1983) alerted us to the richness of Renaissance Aristotelianism. See also Mercer (1993); Martin (2014, chs. 3–5) and Hattab in this volume (ch. 3). These commentators emphasize the creative vitality of *early modern* Aristotelianism, and reject the negative caricatures that have characterized earlier treatments. Raphael (2017) has shown how Galileo's work on motion was received in the schools in Italy, and Ariew (2011, ch. 6) how the observation of the phases of Venus was accommodated to an Aristotelian/Ptolemaic universe.

American politics, some liberal Republicans and some conservative Democrats might look virtually indistinguishable when you compare their beliefs, their favored programs, their political philosophies. But even so, the one group would still identify as Republicans, and the other as Democrats. In this case, the differences are less a matter of political philosophy, and more a matter of how they fit into society, how they relate to one another and to the larger society, the institutions that they do and do not identify with, what clubs or churches they belong to, what books they read, their larger vision of the world, who their friends are, and, even more importantly, who their enemies are. While their beliefs may look similar when taken narrowly, the larger context is everything. So it is with the parties of the *novatores* and the Aristotelians.[78] We will return to this question shortly.

Novatores: Some Reflections on the Category

There is a standard historiographical distinction between actors' and analysts' categories; the former are used by the actual participants in the historical events under study, while the latter are helpful for understanding the events in question, whether the participants used those categories, or even could have used them, or not.[79] How should we think of the category of *novatores*?

The answer turns out to be rather complicated. The term is certainly a kind of actors' category: it is found in the literature of the period, and not infrequently. But even in the period, there was some skepticism about grouping these figures together as if they formed a kind of school of thought. Gerardus de Vries was a professor of philosophy at the University of Utrecht, loyal to Aristotelian philosophy and profoundly hostile to newer currents, particularly Cartesianism. In a public disputation given October 27, 1683, the second in a series titled *Introductio historica ad philosophiam Cartesii*, de Vries set out his own historiographical view of the previous half-century in philosophy.[80] On his view, in "the last fifty years, the divided kingdom of the philosophical world has contained these three sects . . . the Scholastics, the more liberal Peripatetics, and the Ramists."[81] The first is what was taught in the Catholic universities, the second in the Protestant universities, and the third in the gymnasia. But this is what he had to say about the *novatores*:

[78] Here I am in sympathy with Roux (2013a, 55–58) about the distinction between the Aristotelians and the moderns.
[79] On this distinction, see, e.g., Collins (2008).
[80] Vries (1683–1686).
[81] Vries (1683–1686, II.xiv).

> Concerning the Patrizis, the Gorlaeuses, the Keplers, the Galileos, the Bassons, the Telesios, the Campanellas, the Boots, the Bacons, the Gilberts, the Berigards, and numerous other *novatores*, I will not add even a word, since this whole bunch never came together into a unity so as to make up a proper sect.[82]

De Vries was, in a sense, correct. One might be tempted to think of the *novatores* as a kind of alternative party to the Aristotelians. But there was an important difference. As different as the Aristotelians might be from one another, they had texts in common – e.g., *De anima* and *Physica* in natural philosophy. The *novatores* only shared the rejection of the authority of Aristotle and the Aristotelians; beyond that, there was little in the way of a common thread. Telesio explains everything in terms of hot and cold, Gilbert explains everything in terms of magnetism. Others, like Basson, Gorlaeus, and Gassendi, are some variety of atomist. Galileo is not a natural philosopher at all, in the sense of someone offering a system of explanation, though he offers non-Aristotelian doctrines of cosmology and motion. Figures like Bacon, Galileo, and Descartes are usually grouped together as a kind of "progressive wing" of the new philosophers, what many twentieth-century commentators have in mind when they talk about "The New Philosophy." But when we examine them more carefully, we have to acknowledge that their programs are quite distinct and different from one another. Though all the *novatores* from Telesio to Descartes and beyond agree in rejecting Aristotle and Aristotelianism, they could hardly be said to form a uniform school of thought, unlike Cartesians or Gassendists, who arguably do.[83]

A second important feature of the term '*novatores*' is that it was first introduced in the 1620s not by those who belonged to the supposed category, but largely by those who opposed novelty in natural philosophy, such as Mersenne and Frey. Indeed, it continued to be used largely by the opponents of novelty through most of the rest of the century. Eventually, it was used in a somewhat more neutral or even positive way by commentators like Sorel. But while there were numerous attacks on the *novatores* as a group, and some defenses of innovation in natural philosophy, I know of relatively few general defenses of the *novatores* as a collective group. And while many of the figures associated with the *novatores* quite proudly announced their innovations,

[82] Vries (1683–1686, II.xiv). See Dibon (1990, 203).
[83] On this, see Bellis in this volume (ch. 5).

relatively few bragged about belonging to the group.[84] This is the attitude that Descartes took toward the *novatores* in a letter he sent to his former mentor, Isaac Beeckman, in 1630:

> Plato says one thing, Aristotle another, Epicurus another, Telesio, Campanella, Bruno, Basson, Vanini, and the *novatores* all say something different. Of these people, I ask you, who is it who has anything to teach me, or indeed, anyone who loves wisdom?[85]

While Descartes did not lack confidence in his own talents and originality, he seemed to have nothing but contempt for the *novatores* as a group, despite the fact that he was later to be numbered among them; indeed, to become one of the most visible.[86]

Where does that leave the question? The fact that there is little or no coherence in the group suggests that the term *novatores* can have little use as an analysts' category. And while it was certainly used among actors in the period, it also seems strange to consider it as an actors' category, insofar as it was used much more by its opponents than by its supposed members. Rather than debating about actors' and analysts' categories, I suggest that we consider a different kind of category, what might be called a polemical category.[87]

The idea of the *novatores* as a group finds its place in seventeenth-century philosophical and scientific thought in the debate over Aristotelianism. There really were individuals who challenged the Aristotelian philosophy that was generally taught and accepted in the schools. But the idea that they

[84] In the preface of Book II of his *Paradoxes*, de Clave places himself among a number of the *novatores*, including Patrizi, Basson, Campanella, and Gassendi, as anti-Aristotelians (Clave 1635, 185). And then there is the exchange with Thomasius in which the young Leibniz enthusiastically counts himself among the *novatores*. But in general, thinkers who are often listed as *novatores* seem reluctant to claim that status for themselves.

[85] Descartes (1984–1991, 3:26–27); Descartes (1996, 1:158).

[86] See Garber (2015a).

[87] In many ways the idea of *novatores* as a kind of polemical category resembles the category of *libertines*. As with the idea of the *novatores*, the *libertines* have their early modern origin in theological contexts. The term seems to have been coined by Calvin to name a group of his opponents; which is to say, it was used not by the people whom it names, but by their opponents, and was connected with a polemical controversy. It retained its theological meaning through the seventeenth century. In the first edition of the *Dictionnaire de l'Academie Française* (1694), the primary meaning is "[One] who acts too freely and fails to assiduously do his duty." But it also meant "licentious in religious matters." However, in the seventeenth century, like the *novatores*, the *libertini* had become a category of philosophers. This, for example, is what Thomasius calls the *novatores* who do not even pretend to follow an ancient model, as discussed above; see the title of his first lecture against the *novatores*, *"Adversus philosophos libertinos"* (Thomasius 1693, 437). See Cavaillé (2009); Cavaillé (2012).

constituted a group seems to have been the creation of the Aristotelian side, a way of personifying their opponents and giving them a corporate identity. Individually they were all over the map; together they made a target easier to attack. Their faults could be diagnosed in a unified way and they could be dismissed as a group, rather than having to be treated individually. They could be dismissed as seekers of novelty rather than truth, as people who undermined religion and led ultimately to dissent and conflict.

One can argue that the Aristotelians had more unity than the *novatores*. Though there was considerable diversity in their views, they did have common doctrines, and common texts. But the many disputes among the Aristotelians often made it difficult to pin them down on issues. The members of the party of the *novatores* used the category of Aristotelianism in much the same way that the Aristotelians used the category of *novatores*, as a convenient label that could make them a common target of criticism, ultimately leading to their dismissal. In the hands of the *novatores*, "Aristotelian" also was a kind of polemical category, a way of turning a rich and diverse group of thinkers into a simple target.

This connects with what I argued above: the *novatores* (and the Aristotelians) are, in a way, like political parties, categories not defined by doctrine alone, but by a host of other factors. In both cases, we are dealing with collections of individual thinkers who have considerable differences from one another. But despite intra-party diversity, both groups agree on who they oppose. A central thing that united the *novatores*, for the most part, was that they all saw themselves as opposed to Aristotle and Aristotelianism. And while the Aristotelians may have had more in common doctrinally, central to their identity was that they opposed the *novatores*. There is more to it than that, of course. The Aristotelians tended to cite similar authorities, including one another. Many of them were also associated with universities and taught students. The *novatores* also tended to live in their own intellectual world, largely distinct from that of the Aristotelians; they generally did not cite Aristotle, but did cite a variety of other figures, including other ancients and one another. And while there were *novatores* who were associated with universities,[88] many and perhaps most were not. But in many ways, the opposition between Aristotelians and *novatores* is central to defining both.

The conflict between the *novatores* and the Aristotelians is, to an extent, a conflict between two caricatures. The opponents pick out the most

[88] De Raei, for example, taught at Leiden and Amsterdam, and the Cartesian Johannes Clauberg taught at Herborn and Duisburg.

objectionable features of the other side, and focus their attacks on those as if they are true of all members of the group. For the *novatores* against the Aristotelians, it is the latter's attachment to tradition and authority, rather than the truth. For the Aristotelians against the *novatores*, it is the arrogance of the *novatores* and their love of fame and novelty over truth. These caricatures are not altogether false. Some number of the Aristotelians were, indeed, dogmatic. And some number of the *novatores* sought fame. Hobbes, for example, claimed that "since the authority of Aristotle is only current there [i.e., in the Schools], that study is not properly philosophy . . . but Aristotelity."[89] In response to Hobbes's charge, Seth Ward replied in his *Vindiciae academiarum* (1654):

> it is manifest, that the only thing which paines him is the desire that Aristotelity may be changed into Hobbeity, & insteed of the Stagyrite, the world may adore the great Malmesburian Phylosopher.[90]

This was not an empty charge. In the penultimate paragraph of the *Leviathan*, Hobbes wrote:

> I think [this book] may be profitably printed, and more profitably taught in the Universities, in case they also think so, to whom the judgment of the same belongeth [i.e., the sovereign].[91]

Similarly, there is good reason for thinking that Descartes hoped that his philosophy would replace Aristotle and be taught in the Jesuit schools.[92]

Though the caricatures are not altogether false, they are not altogether true either. While Frey refuses to attribute any erroneous doctrine to Aristotle, Mersenne, in his defense of Aristotle in *La vérité des sciences* (1625), goes out of his way to present a long list of Aristotelian doctrines with which he disagrees.[93] And while both Hobbes and Descartes looked for fame and acceptance, it is very difficult to believe that neither of them were interested in finding new truths and advancing knowledge.

The complex social and political context and nature of the debate between the *novatores* and the Aristotelians is nicely captured in a contemporary observation. In 1690, Father Gabriel Daniel published a curious work,

[89] *Leviathan*, ch. 46 (Hobbes 2012, 1074). [90] Ward (1654, 58).
[91] *Leviathan*, "Review and conclusion" (Hobbes 2012, 1140).
[92] The *Principia philosophiae* (1644) was explicitly written in a way to make it easy to teach in the schools. See Garber (1992, 25).
[93] Mersenne (1625, 119–125). Even so, Mersenne considers that Aristotle's principles "are more probable than the others," and that "Aristotle is an eagle in philosophy, and the others are like chicks, who wish to fly before they have wings" (Mersenne 1625, 109–110, 125).

Voiage du monde de Descartes.[94] The book was a fantasy in which Descartes's soul detached from his body, and, unable to re-attach, continued to wander through the world as Descartes imagined it in his *Le monde*. This enables the narrator, a stand-in for Daniel himself, to have imaginary conversations with Descartes. At one point, the fictional Descartes asks the narrator for news about the current state of philosophy in his world, the real world that Descartes had left some years before. This is the answer he receives:

> Your philosophy, as you know, was set up with all the Advantages and Disadvantages of *Novelty*, and it has experienc'd the Fortune which all *New Doctrines* use to find. Many there are that have imbrac'd it with Admiration, and defended it with Earnestness and Passion. It hath met with the Patronage and Protection of Persons commendable for their Parts, Capacity and Politeness; but almost all *Bodies* and *Universities* have rejected it, and declared against it: Each acted in that as in all things else, according to the Principle of Self-interest: Some to your Side, as apprehending themselves thereby distinguish'd and advanc'd above the Herd; others deny'd it, as fearing the diminution of their credit: The Motive and Pretence of both Parties was the Love of truth and uncorrupt Doctrin.[95]

Does this mean that the debate between the *novatores* and the Aristotelians was just shadow boxing, mere polemic, politics, and expressions of self-interest with no intellectual content? To be sure, both sides distorted their opponents' views in a debate that often smacked of posturing and polemic. But even so, there was also real intellectual debate and genuine disagreement. It is difficult to doubt that there was a core of thinkers in the party of the *novatores* who genuinely thought Aristotle was wrong, and were trying to carve out a genuine alternative philosophy. And there was a core of thinkers in the party of the Aristotelians who genuinely believed that Aristotle got much right, and that the Aristotelian philosophy, in one form or another, leads to truth and piety. Despite the polemics, there really was a debate. To understand the debate fully, we must recognize the posturing and misrepresentation, on both sides. But we must also see below the surface and appreciate the complexity of the transformation. There were real issues.

Concluding Thoughts

The Reformation was a traumatic event for European Christianity. While Luther, John Calvin, and Huldrych Zwingli were, perhaps, the most

[94] Daniel (1690). See Smith (2019). [95] Daniel (1690, 266–267); Daniel (1692, 176–177).

prominent, a host of reformers challenged the hegemony of Rome both before and after them. From these sprung Lutherans, Calvinists, Anabaptists, Mennonites, Quakers, and many other sects.[96] The Roman Catholic Church remained, but the Christian community in western Europe was fragmented into a variety of sects that competed with one another. While the list of sects evolved and changed over the years, despite schemes (like Leibniz's) for reuniting the churches, the fragmentation persisted.

The so-called Scientific Revolution of the seventeenth century was, in a way, not unlike the Protestant Reformation. As with the Reformation, there was one main target against which the reformers directed their attention: the Aristotelianism of the schools. And as with the Reformation, there soon arose a variety of very different alternatives to that main project, as can be seen in the diversity of the *novatores*. By the time of figures like Bacon, Descartes, and Galileo, often designated as the fathers of the new philosophy and science, there already was a "swarm of innovators" in battle with each other, each one attempting to be the one to replace Aristotelian orthodoxy. And just as the Catholic Church survived the Reformation, the Aristotelian school philosophy continued to persist in the schools and universities.

In a political revolution, when one regime is overthrown, there is a strong incentive to replace it as soon as possible with a stable new regime. There is a sense in which it must be this way: people need stability in which to live. But science does not necessarily work like that. There was tremendous diversity among the *novatores*: they offer not a single new program, but a variety of alternative programs. The *novatores* were in competition with one another just as much as they were in competition with the dominant philosophy of the schools. Nor is this a bad thing for science and philosophy. People who are forced to live in a failed state, in essence a Hobbesian state of nature, suffer. Without a stable social environment, it is difficult to accomplish anything. But the scientific world is entirely different. There, competition is not only tolerated, but welcome. We correct and strengthen our views through the intellectual competition of competing points of view.[97]

There is a popular view that modern science and philosophy had a father, a figure who gave rise to a new view of the world. For some it is Bacon, for others Galileo, or Descartes, or a combination of figures. But in reality, modern science begins with the *novatores*, a long, diverse, and constantly evolving list of thinkers who were attempting to break free of Aristotelian

[96] See Rublack (2016, pt. 2).

[97] This argument is taken from Garber (2016b), where it is developed at greater length in connection with Kuhn's account of scientific revolutions.

tradition and forge new paths in natural philosophy. As the century progressed, the once long list of innovators became shorter and shorter as many of the figures that once occupied places of honor (or infamy) fell out of fashion and become forgotten. And so, for example, in his *Reflections on Ancient and Modern Learning* (1694), William Wotton could claim that "My Lord *Bacon* was the first great Man who took much pains to convince the World that they had hitherto been in a wrong Path," followed by "Monsieur *DesCartes*, who came soon after," after whom came the heroes of the Royal Society of London, including Robert Boyle, Isaac Newton, Christiaan Huygens, and others.[98] Gabriel Daniel notes something even stronger. In the *Voiage*, his character tells the fictional Descartes that:

> you have had the good Fortune with your Lustre to efface all the New Philosophers that have risen both in and since your Time. . . . As in *Spain* the Name of *Lutheran* is indifferently given to all *Hereticks* of whatever Sect or Faction, so the Title of *Cartesian* is attributed to all those that have undertaken to make Refinements in Point of Natural Philosophy . . . .[99]

In this way a creation myth is formed, and the true origins of modern philosophy and science in a chaos of competing *novatores* came to be forgotten.[100]

[98] Wotton (1694, 306–307). [99] Daniel (1690, 268); Daniel (1692, 178).

[100] Many thanks to Dana Jalobeanu and David Marshall Miller for the extensive comments that they made on earlier drafts of this chapter. It is much improved because of their care and attention.

3

Renaissance Aristotelianism(s)

HELEN HATTAB

Historians of philosophy and science long presumed not only that there was one coherent Aristotelian philosophy of the Schools, but also that it was, at worst, antagonistic to innovations in the sciences and, at best, indifferent. It is still commonly presumed that the theologically tinged commitment to fundamental Aristotelian metaphysical principles, especially the elements of Aristotelian substance theory used to account for Christ's nature, the Eucharist, and personal immortality, posed obstacles to progress in natural philosophy. Hence, the advent of *modern* science often still is identified with the seventeenth-century rejection of Aristotelian substance theory in favor of anti-Aristotelian matter theories amenable to the mechanization of nature. This aligns with the traditional narrative of rupture from Aristotelianism, which features two mutually exclusive camps locked in an epic power struggle: the mechanists championing mathematical physics and experimentation versus the Scholastic Aristotelians, desperately clinging to the authority of Aristotle and religion. This picture misleads, engendering a methodology that presupposes the Renaissance to be mere prelude to a sudden shift known as the Scientific Revolution.

Recent scholarship has rehabilitated the Scholastic Aristotelian tradition, showing that it was neither monolithic nor consistently beholden to Aristotle and religious authorities. Medieval developments in the Aristotelian mixed mathematical sciences of astronomy and optics are well known, while recent work shows that Aristotle's accounts of motion and mixtures were also developed in innovative ways by late Aristotelians.[1] The roles that the Renaissance recovery of the *Quaestiones mechanicae* (then thought to be by Aristotle) and Archimedean mechanics played in further extending mathematical demonstrations to physical phenomena, and the role that the study of

[1] Di Liscia (2014); Petrescu (2014).

Aristotle's biological writings played in the emphasis on observation and experimentation in medicine have also received attention.[2] Some of these influences will be discussed in subsequent chapters. However, given that the contributions of the Aristotelian tradition tended to be incremental rather than seismic, most are overlooked.

Anglo-American research on Renaissance Aristotelianism, in particular, is still driven by the search for direct links to the methods and doctrines of canonical modern figures. This fosters a hunt-and-peck methodology that limits forays into Renaissance texts to apparent points of contact. Notwithstanding some recent more comprehensive work,[3] we still have a gappy, lopsided picture of intellectual developments during the Renaissance that both overstates some connections and overlooks others. E.g., there has been extensive research on Zabarella's *regressus* and its relation to the methods of Galileo and Hobbes, but few studies of Aristotelian texts on method produced outside Padua's orbit.[4] This fosters confusion, such as the hasty identification of Hobbes's methods of analysis and synthesis with the phases of the *regressus*.[5] Similarly, Aristotelian mixture theory is studied in relation to atomist and corpuscularian matter theories, but how the core concept of prime matter was interpreted and developed by Renaissance Aristotelians is understudied, the presumption being that change came from incorporation of anti-Aristotelian doctrines.[6] Hence, increased support within Aristotelian circles for the view that prime matter does not get its existence from form, but is self-subsistent, gets overlooked.[7]

While the prevailing methodology has made some progress, given its selective focus on particular domains, we must refrain from drawing general conclusions about the nature and contribution of Renaissance Aristotelian philosophies. The gappy and slanted state of current knowledge makes an overview of this tradition and its importance premature.

Further complicating matters is the fact that the precise meanings of both terms in my title are disputed. It is unclear when there was a Renaissance in philosophy and what it entailed. If it consisted in the advent of humanism, then the Renaissance might appear to begin with Petrarch in fourteenth-century Italy and to end before the seventeenth century. However, humanist endeavors in Italy predated Petrarch and spread to parts of Northern Europe later, such that, by this criterion, the Renaissance lasted longer than

[2] Cunningham (1985); Laird (1991); Hattab (2005); Lennox (2006b); Hattab (2009).
[3] Des Chene (1996); Des Chene (2000a); Blum (2012b); Martin (2014).
[4] Kuhn (1997); Palmieri (2017). [5] Hattab (2014, 462–472). [6] Lüthy, et al. (2001).
[7] Hattab (2021).

commonly thought.[8] When one adds the term 'Aristotelianism', temporal divisions get even murkier. Despite the influence of humanism and establishment of chairs of Platonic philosophy in Italy during the Renaissance, university education remained firmly grounded in Aristotle's works. The high degree of continuity between medieval Scholastic Aristotelianism and what was later taught in the schools makes it problematic to speak of a "rebirth" of ancient philosophy, even if humanism and renewed interest in Platonic and Hellenistic philosophies and commentaries affected how the texts of Aristotle were translated, interpreted, and taught.[9]

Another complicating factor is that, although European university education was overwhelmingly based on Aristotle's texts, there was not a unitary, consistent set of doctrines and approaches associated with Aristotle.[10] Diverse educational contexts with differing priorities produced a rich variety of interpretations and uses of works attributed to Aristotle. Marco Sgarbi also demonstrates that self-attributions are unreliable as there was no need to self-identify as an Aristotelian during this period,[11] while Sgarbi and I show that even Renaissance philosophers who explicitly attack Peripatetics for blindly following the authority of Aristotle do not always reject fundamental Aristotelian methods and doctrines.[12] The line between one who aims to discredit Peripatetics to reveal Aristotle's true doctrine and one who rejects Aristotle altogether is thus blurred.

Additional confusion stems from the boundaries within the Aristotelian hierarchy of disciplines. Mathematicians commenting on the *Quaestiones mechanicae* and seeking to elevate the art of machines to a science grappled with different issues than natural philosophers lecturing on Aristotle's *Physics*, who, in turn, had diverging aims from theologians confronting tensions between Aristotle's metaphysics and religious doctrine.[13] Moreover, Aristotle is often unclear, so systematic thinkers took positions at odds with claims in one part of his corpus to preserve consistency among other passages and doctrines. Whether a particular Renaissance figure looks Aristotelian or anti-Aristotelian depends on which level of the disciplinary hierarchy and which Aristotelian texts plus medieval interpretation we privilege. In addition to variations by institutional context and discipline, there were individual differences, since faculty holding equivalent positions at the same institution could disagree.

[8] Hankins (2007, 41). [9] Bianchi (2007). [10] Schmitt (1984a); Schmitt (1989).
[11] Sgarbi (2017). [12] Hattab (2017, 441–447, 455); Sgarbi (2017, 228–230).
[13] Hattab (2009, 93–98, 223–225).

Altogether, these confounding factors force us to speak provisionally of "Renaissance Aristotelianisms" and, pending further research, leave open whether they share a common core or are bound by other ties that allow us to group them together. For now, I employ Edward Grant's broadest characterization of Scholastic Aristotelians as intellectuals who take as their starting point Aristotelian texts and principles, regardless of their interpretations of and degree of assent to Aristotle's texts.[14]

Texts in Contexts

To avoid prematurely generalizing and to sidestep the imprecise conception of "Renaissance Aristotelianism," my aim in this chapter will be to illustrate how we can improve our knowledge of the intellectual situation in the period by studying texts and issues crucial to Renaissance Aristotelians working in different disciplines at a variety of institutions, independently of the presumed hallmarks of modernity privileged by the traditional narrative. This approach yields a more balanced and nuanced picture of developments within the Aristotelian tradition, preparing the ground for more informed connections to seventeenth-century figures, methods, and doctrines.

In particular, I here examine three different educational contexts: the University of Padua in the sixteenth century; the Jesuit Order; and the Calvinist Academy, which became the University of Leiden in the newly established Dutch Republic. Focusing on a prominent text from each context, I show that each treats a core issue in Aristotle's natural philosophy differently, while still remaining within an Aristotelian framework. Each treatment yields a distinct position on substantial generation and the relationship between prime matter and substantial form. The positions are paralleled by a progressive distancing from Aristotle's text, ranging from a defense of Averroës' interpretation, to an interpretation that makes room for artifacts undergoing substantial change and for matter as a *quasi*-independent partial substance, and finally to an outright rejection of substantial change and hylomorphism. Contrary to common assumptions, innovations that anticipate seventeenth-century substance theories come from the more theologically motivated Scholastic authors, not the "radical Aristotelianism" of Padua.

My first text is Giacomo Zabarella's *Commentaries on the Books of Aristotle's Physics*, based on his lectures and posthumously published by his sons in its

[14] Grant (1987c, 342). The difficulty of categorization is exemplified by the case of Gorlaeus – compare the treatment below with that by Garber, also in this volume (ch. 2).

incomplete form in Venice in 1601. Zabarella (1533–1589), whose entire career from student to professor was spent at Padua, taught Aristotle's works as a professor first of logic and then of natural history. Best known for his works on logic and method, which were immensely influential in both Catholic and Protestant Europe, his physics represents a naturalistic approach to Aristotle inspired by Averroës and associated with the medical school at Padua.[15] In format, his commentary on the *Physics* resembles medieval commentaries on Aristotle's works, which divide the Latin translation of Aristotle's text into smaller parts, and then systematically explicate each part more or less line-by-line. Unlike medieval commentators, Zabarella had access to the Greek manuscripts of Aristotle's works, and he refers to them, as well as Hellenistic commentators. Consequently, Zabarella divides Aristotle's text into even shorter passages and provides more extensive commentary on each part than one finds, for example, in Thomas Aquinas's commentary.

My second text is the *Commentary* on all eight books of Aristotle's *Physics*, which formed part of a series of Aristotelian commentaries composed by the Jesuit faculty of the University of Coimbra, known collectively as the Coimbran commentators. Their commentaries were possibly the most frequently printed and widely diffused natural philosophical works by Jesuits.[16] Medieval commentaries usually relied on poor Latin translations of Aristotle's texts and did not clearly distinguish between the views of the historical Aristotle and the interpretations of subsequent commentators. The philological achievements of Renaissance humanist scholars gave rise to the need for improved translations and more informed commentaries. The Coimbran commentaries fulfilled this need by providing not only an improved Latin translation of Aristotle's texts, but the original Greek alongside it. Furthermore, the views of other ancient philosophers and commentators are carefully identified in the detailed questions that follow the original text and commentary. As this commentary was published in 1594, just three years before Francisco Suárez's very influential *Metaphysical Disputations*, I will highlight overlapping content in these two influential Jesuit works.

My last text is the *Exercitationes philosophicae* by David Gorlaeus (1591–1612), a relatively obscure theology student at the University of Leiden during the Arminian Crisis. But for his atomism and his later association with the Arminian and Socinian heresies and with Cartesianism, he would have fallen into oblivion. Gorlaeus gained notoriety through two works published after his death at age 21. The *Exercitationes philosophicae*, published by his family in

[15] Mikkeli (1992). [16] Martin (2014, 95).

1620, most likely dates back to 1610.[17] This consists of a series of academic exercises on standard topics in Scholastic metaphysics and physics that theology students had to master. Such student exercises were practice disputations, and were commonly published by scholars at Protestant universities.[18]

I first turn to Zabarella and the Coimbrans to illustrate how their explications of Aristotle's text serve to introduce and motivate a diverse range of philosophical questions that, while related to the text, derive from broader, extra-textual concerns. To provide depth, I focus on a passage in Book I of Aristotle's *Physics* central to Aristotelian reflections on substantial change and the relationship between prime matter and substantial form. After examining how this passage is explicated by Zabarella and the Coimbrans, respectively, I turn to the concerns and arguments each commentator raises in connection with it. Finally, I discuss Gorlaeus's *Exercitationes*. The only text under consideration written after Suárez's *Metaphysical Disputations*, it provided a systematic treatment of standard issues in Aristotelian metaphysics and natural philosophy. Following a trend in late Scholasticism, Gorlaeus does not comment on Aristotle's text but treats stock Scholastic topics and questions systematically. Correspondingly, Gorlaeus's treatment of substantial change differs dramatically from that of the commentators. He commonly is assumed to have been motivated by an anti-Aristotelian commitment to atomism, but this ignores the fact that Gorlaeus argues for atomism from Aristotelian principles.

The passage addressed by Zabarella and the Coimbra Jesuits forms part of Aristotle's argument in *Physics* I.7[19] that there are three principles in every natural change: matter, form, and privation. The argument begins with the distinction between generation of a substance – substantial change – and alteration – accidental change. It infers that, just as there is a subject of every accidental change, e.g., the substance that becomes hot, there must also be a subject in the case of substantial change, i.e., prime matter. This passage is a *locus classicus* for a host of questions regarding prime matter and substantial form. In sixteenth-century Jesuit commentaries and textbooks, it becomes the occasion for debating the theologically important question whether or not matter can have being without form.

The passage in question appears as Text 62 in Zabarella and Text 61 in the Coimbran commentary:

> Text 62: However when [something] is said to come to be, [the expressions are] multiplied, and among these [it is] indeed [said] not that [something]

[17] Lüthy (2012, 11, 27). [18] Marti (2001). [19] 190a31–190b4 (Aristotle 1984, 1:324–325).

> comes to be *simpliciter*, but that something becomes this; however to come to be *simpliciter* is only [said] of substances. It is manifest that in accordance with the others it is indeed necessary that something underlies what comes to be, for how much, what kind, relatedness to another and when and where come to be from a certain subject. Therefore substance alone is said to come to be from no subject, but all others from substance. However that substance, and whatever other beings [are] *simpliciter*, come to be from a subject would in any case become manifest in being considered, for there is always something underlying, from which what came to be would be, such as plants and animals from seed.[20]

> Text 61: Since however, the same thing can be said to come to be in many ways and some things are not [said to] come to be but to come to be something, substance alone is [said] to come to be entirely. Indeed in the others it is established that something necessarily underlies, namely that which comes to be.
>
> For how much, and what kind, and what is referred to something else, and when, and where come to be in a certain thing underlying, since substance alone is not said regarding another subject; however, all the rest [are said] regarding it. But by directing the mind it would be manifest that substance too, and all other things which are entirely, come to be from a certain subject. For there is always something which underlies from which that which originates comes to be, as plants and animals from seed.[21]

Setting aside some differences in phrasing, the first two sentences appear consistent with each other. The second to last sentence in the Coimbran text and the last one in Zabarella's refers to other beings besides substance. Zabarella's text says such beings exist *simpliciter*, whereas the Coimbrans render this in terms of other things that come to be "entirely." The terminological difference does not seem to affect the meaning of this sentence, but both Zabarella and the Coimbrans discuss a doubt it raises.

The Coimbran commentary concisely lays out two available interpretations of the sentence. The first is the ancient commentator Alexander's reading, which takes "all other things which are entirely" to include indiscriminately the products of all modes of generation, both substantial and accidental. On the other reading, which is not attributed to anyone in particular, the clause refers more specifically to other things generated *simpliciter*, such as artifacts. Artifacts do not have the form of a substance, but they are not said to come to be this or that, so they constitute another kind of thing that comes to be entirely. The authority of Saint Thomas Aquinas, who writes elsewhere that artifacts are in a way in the genus of

[20] Zabarella (1601, 97^{v}–98^{r}). [21] Conimbricenses (1984, 133).

substance due to their matter, is invoked in support of this reading (the reference is to Book I, Lecture 12, Section 108 of Aquinas's commentary on the *Physics*[22]). Further support is found in Aristotle's *Physics* I.7,[23] where he states that just as the matter of a thing is a part of the whole, the subject from which an artifact is composed is also its part. This implies that the generation of substances and artifacts could proceed in like manner, from a composition of their parts. The Coimbrans do not choose explicitly between the two interpretations, stating that both are pleasing, though they note that the ancient commentator Simplicius rejects the first.[24] Hence, they allow and even perhaps incline toward the second view. In fact, this reading is necessitated by their subsequent arguments, which reinterpret prime matter and substantial form as incomplete substances each capable of being conserved independently by God. With this contrast in mind, I turn to Zabarella's commentary.

Zabarella

Unlike the Coimbrans, Zabarella follows Averroës, who suggests that the clause is declarative not conjunctive. In other words, it should read, "substance, i.e., all entities beyond accidents that are *simpliciter*," rather than "substance and all other entities that are *simpliciter*." Averroës also suggests it could be conditional and thus read, "substance, and if these others are being *simpliciter*."[25] In either reading, the phrase refers exclusively to substance, supporting Zabarella's intent to rule out the artifact interpretation, which he attributes to "Vimercatus and certain others."[26]

Zabarella offers three reasons to reject reading any reference to artifacts in the passage. His first is that this would be inconsistent with the fact that Aristotle does not include artifacts in the first line of the passage, but rather states that only substances are said to come to be *simpliciter*. However, it seems odd for Zabarella to put so much weight on this line, since he had pointed out an error based on the Greek codex earlier in his commentary.

Zabarella's second, more philosophical argument against the inclusion of artifacts is that Aristotle distinguishes between substantial and accidental generation on the basis that all accidents come to be from a subject that is manifest, because it is in act and sensible. Then he claims that in substantial generation – that is, for beings that come to be *simpliciter* – the subject is not manifest, but has to become manifest through consideration. There is no

[22] Aquinas (1963, 57). [23] 190b10–15 (Aristotle 1984, 1:325).
[24] Conimbricenses (1984, 133–134). [25] Zabarella (1601, 99). [26] Zabarella (1601, 98^{v}).

doubt, then, that artifacts come into being accidentally, since they each have a subject that is in act and sensible. Zabarella concludes that what comes to be from art only comes to be by accident, despite the fact that artifacts are given the names and definitions of things. Artifacts, insofar as they exist outside the mind, are said to be substances and bodies only by virtue of their natural matter. So artifacts are *simpliciter* insofar as they are made of natural matter, but insofar as they are diverse from substances (by virtue of having an artificial form, I presume), they are not called beings *simpliciter*.

This seems like a rather spurious reading of the text. All Aristotle says is that it requires some attention to see that there is a subject in substantial change, whereas it is immediately clear in accidental change. Nothing in Aristotle's text implies that it is *essential* to substantial change that the subject be hidden and to accidental change that it be sensible. Zabarella's reading appears designed to preserve a strict division between natural and artificial change, and along with that, another between natural substantial forms and artificial forms. Unlike the Coimbran reading, it precludes the possibility of a concrete, sensible subject of generation.

Zabarella's reading of this passage seems colored by the way he developed Aristotle's theory of scientific demonstration in the *Posterior Analytics*. In his writings on the subject, Zabarella elaborates the procedure known as *regressus*, which is a means of discovering hidden principles from known effects. In so doing, he takes the above-cited passage in Aristotle's *Physics* to be an instance of *regressus* proving the universal proposition that in every generation it is necessary that there be a subject (i.e., matter).[27] Likewise, Zabarella uses the *regressus* method in a treatise on prime matter.[28] That the *regressus* is required to uncover it implies the hidden nature of prime matter. Meanwhile, unlike contemporaneous mathematicians, Zabarella makes no attempt to fit demonstrations of the workings of machines into the scientific method of the *Posterior Analytics*, again distinguishing substantial from artificial generations.[29] (Notably, the parallels between Zabarella's texts indicate he was developing a systematic Aristotelian natural philosophy to unify Aristotle's *Physics* and *Posterior Analytics*.)

The last reason Zabarella gives for rejecting the artifact reading is that Aristotle's example in the very next sentence (the last one quoted above) is about natural substances; namely, plants and animals coming from seed. He clarifies that Aristotle is saying that the seed, as material subject, does not remain in the generated animal or plant. Rather, 'seed' signifies three things:

[27] Zabarella (1597, 98). [28] South (2005). [29] Hattab (2009, 110–119).

the form of the seed, the material subject, and the privation of the form of the generated animal or plant, which Zabarella says, while discussing the generation of a human being, "is perpetually annexed to the form of the seed."[30] Thus, in substantial generations, the form of the seed carries a necessary connection to the privation of the generated form. Zabarella then goes on a lengthy digression regarding Averroës' account of how the substance generated comes to be from the seed, the gist of which is that the parts of the form of the seed gradually and successively recede, while gradually the parts of the form arrive until it is whole and complete. So once the form is complete, the whole form of the seed is corrupted. This contrasts with the generation of artifacts, in which the subject matter remains in the product. For instance, the wood is still present in the bed or house from which it is made. Therefore, Aristotle's example cannot comprise the generation of artifacts.

Zabarella spends the rest of his commentary on this passage defending Averroës against those who interpret him as holding that the substantial form can intend or remit, and therefore that it is divisible. Again taking the case of human generation, Zabarella argues that the introduction of the human form occurs in an instant, so generation is not a motion. However, the introduction of the human substantial form can only occur after the matter is prepared to receive it. There is therefore true motion in the qualities of the matter, as the accidents of the seed are removed by remission, and the accidents of a human being are intended and gradually arrive by degrees. So whereas the *terminus* of the generation, the introduction of the substantial form of an organism, occurs in an instant, the previous alteration, where the matter is prepared to take on this form, takes time, and in this sense, generation is motion and has continuity.[31]

Notice that throughout his commentary on the *Physics* passage, Zabarella is concerned with the generation of substantial forms in a purely physical sense. His digressions address details about the physical process of becoming, such as the question whether forms come to be in an instant or by motion, and they also reflect methodological issues relating to the *regressus*. But aside from a brief reference to Aristotle's categories in relation to the contrast between motion and generation, metaphysical or logical considerations about form do not enter into his considerations. In particular, Zabarella does not here or elsewhere in his commentary (at least not that I could find) address metaphysical questions important to theology. For the most

[30] Zabarella (1601, 99^{v}). [31] Zabarella (1601, 99^{v}).

part, Zabarella follows Averroës, including the latter's naturalism, and though he has access to the Greek text, this does not appear to affect his interpretation of this passage very much.

Coimbra Commentators

Craig Martin argues that the Jesuit approach to Aristotle is directly opposed to the approach of Italian naturalists like Zabarella, who separated inquiry into the true meaning of Aristotle's texts from inquiry into the truths of faith.[32] Unlike Zabarella, the Coimbrans aim for consistency with theological doctrine. One expects this, since the Jesuit order was founded to lead the Counter-Reformation, and philosophy was preparatory to further study in theology; whereas at Padua, Aristotle's natural philosophy served as preparation for medical studies. Yet even if Jesuit textbooks were beholden to the overarching mission of the Counter-Reformation, this does not necessarily indicate that the naturalistic approach inspired by Averroës was more important to developments in Renaissance matter theories. Nor does it mean that naturalist approaches always yielded superior interpretations. The Coimbran commentary, for example, interprets the text more carefully than Zabarella. By including a discussion of standard questions, further subdivided into articles, after each passage and its commentary, it also clearly separates philosophical questions raised by the text from the explication of the text. This could reflect a distancing of philosophical inquiries into the true nature of substance from the question of how to interpret accurately Aristotle's claims on the subject. In Zabarella's commentary, by contrast, the text is the occasion for defending, all at once, the interpretation of Averroës and the truth of the view of substantial generation it implies.

Whereas Zabarella focused on natural philosophy, the textually separated philosophical questions in the Coimbran commentary address mostly metaphysical issues. In their treatment of the above passage to the end of *Physics* I, the Coimbrans raise nine questions, each subdivided into as many as six articles. Here, I will concentrate on the sixth question, "Whether matter can exist by divine power without all substantial form or not?"[33] This question is subdivided into three articles. The first lays out the main arguments on either side, reflecting the discussions of medieval and more recent authorities. The proponents of the negative view that matter cannot exist without a substantial form are more numerous, indicating that it was dominant,

[32] Martin (2014, 99–100). [33] Conimbricenses (1984, 167).

and the arguments listed on this side of the question include seven main arguments, against three main arguments on the affirmative side. The second article gives a more accurate explanation of the foundation of each opinion, discusses the cause of existence, and establishes that matter does not take existence from form. The third article concludes in favor of the affirmative side of the question – matter can exist without any substantial form – and responds to objections. The entire discussion concludes with an argument that even though God could conserve matter without form, matter has, in fact, never existed without form. Suárez later makes many of the same arguments to show that matter is a partial, incomplete substance that does not exist naturally without form.[34]

In support of their conclusion, the Coimbrans offer some theological reasons. For instance, they reject the Augustinian reading of Genesis as implying the pre-creation of unformed matter.[35] Other theological concerns on either side of the question include the continued existence of Christ's body after his death. Here, the Coimbrans adopt a view (which Suárez will reject) that, upon the withdrawal of the soul, the form of the cadaver arrives and accounts for the continued integrity of the body.[36]

Theological considerations also inform arguments in favor of God's ability to conserve matter without form. The third argument given on this side of the question in Article 1 begins with the premise that what is posterior derives more from what is prior than the prior from what is posterior. Since an accident is posterior to substance, whereas matter is prior to form, an accident must depend more on substance than matter on form. But an accident can exist without a substance, as seen in the Eucharist. Therefore, matter can exist without any form.[37] The comparison to the Eucharist is invoked again in the third article to show that God could cause matter to come to be without any form, by lending a greater concurrence than any form achieves in actuating matter. In support of this, the Coimbrans point out that it is the common view of theologians that, in the case of the Eucharist, God supplies the concurrence of the material cause by actively conserving quantity and the remaining accidents outside the subject. Similarly, God's active concurrence could supply the concurrence of formal and efficient causes in the creation of matter.[38] Suárez makes essentially the same argument in *Metaphysical Disputation* 15, Section 9.[39]

[34] Suárez (2000, 115–126). [35] Conimbricenses (1984, 171–172).
[36] Conimbricenses (1984, 172). [37] Conimbricenses (1984, 168).
[38] Conimbricenses (1984, 171). [39] Hattab (2017, 454).

Nevertheless, most arguments the Coimbrans present are not theological, but metaphysical, and in the following I illustrate ways in which the Coimbran commentators, unlike Zabarella, bring in metaphysical considerations drawn from other parts of Aristotle's corpus. For example, Aristotle's metaphysical distinction between potency and act grounds several arguments for the position opposed by the Coimbrans – the Thomist view that matter cannot exist without any form, even by divine power. For Aristotle, matter according to itself is pure potency, and its act is the substantial form. Now, in order to exist, matter would have to be in act. But if matter could exist without any substantial form, it would be in act without an act. This is a contradiction, so this possibility must be rejected. Another argument in this vein begins with the premise that matter derives every act of existence from form. This is supported as follows. Pure act has no potentiality. Given the implicit conception of potentiality as a capacity for form or further actualization, pure act cannot tend toward any further actualization and must, in any natural process of change, tend in the opposite direction to potency. Since pure act and pure potency are opposites, pure potency must correspondingly tend toward act. Consequently, just as pure act has no potency, so pure potency has no act. Accordingly, pure potency would lack all existence. Therefore, since matter is pure potency, it cannot exist without the form.[40] These arguments may sound question-begging, but once one accepts the definition of matter as pure potency, and one understands that act and potency are relative to one another in Aristotelian metaphysics, with act being the active formal principle responsible for existence, then the consequence is inescapable. So, unless one fundamentally revises the Aristotelian act/potency relation, one cannot hold that prime matter and substantial form are incomplete substances that could exist independently and yet call one pure potency and the other pure act, which is the position the Coimbrans and Suárez adopt. What prompts them to undertake such a fundamental revision of this core doctrine of Aristotle's?

The position ultimately affirmed by the Coimbrans – namely, that matter could exist without any form, with the qualification that this cannot happen naturally – also rests on metaphysical grounds. But these grounds stem from theories of individuation and essence, not the act/potency distinction. According to one argument, presented in Article 2 as part of the foundation of this view, the existence of something is not distinguished from essence except in the way that a mode of a thing is distinguished from the thing itself.

[40] Conimbricenses (1984, 167).

That is, there is merely a modal distinction between a thing's essence and its existence. Therefore, just as every individual essence has a difference from others, it must also have proper existence that is distinct from the existence of other things. But matter, just as much as other things, is individually distinguished by essence. Therefore, it must also be distinct in its existence.[41]

As seen in this case, late Aristotelian theories of essence, and the relation between essence and existence, are in competition with the traditional understanding of the act/potency relation. In this competition, at least for the Coimbrans, theories of metaphysical essence in relation to existence win out, such that matter as a pure potency can still exist on its own. Potency is now no longer defined relative to act. We have matter as a definite something that can be conserved in existence without form; it just happens to also be in potency to a substantial form (i.e., have the capacity to be actualized by acquiring such a form) – it is not defined in relation to its corresponding actuality. With this, we lose the dynamic nature of Aristotelian substance theory, where matter and form work in tandem, with the potencies latent at each level of matter being actualized in a continual process of realization. We move instead to a more binary, static way of thinking, where we have a definite though incomplete thing called prime matter, on one side, and another definite incomplete thing called the substantial form, on the other. Matter is still called potency, and form is still called act, but the fundamental interrelatedness of the two is lost, so the meanings of these fundamental Aristotelian metaphysical terms are radically transformed. This transformation occurs entirely within the Aristotelian framework and is not driven by the new sciences. Yet, as I argue elsewhere, it de-emphasizes formal causation, anticipating the rejection of formal causation in favor of material and efficient causation and Descartes's formulation of substance dualism.[42]

Let me sum up my observations about these two commentaries before turning to Gorlaeus. First, the Coimbrans are more careful in their interpretation of Aristotle's text than Zabarella and leave open the possibility that artifacts come into being entirely; i.e., like natural substances, they undergo substantial change consisting in the composition of parts. This is congenial both to their metaphysics of substance and to the concurrent nature/machine analogy of Renaissance mechanics. Zabarella, despite the fact that Aristotelian mechanics was taught at Padua, reads Aristotle's words conservatively, so as to limit substantial change to natural substances, and defends

[41] Conimbricenses (1984, 169–170).
[42] Descartes (1984–1991, 2:54); Hattab (2011b, 156–160); Hattab (2012, 110–117).

Averroës' account of substantial change. The Coimbrans append standard questions raised by medieval authorities to their commentary and break with the Thomist view, which, according to the *Ratio studiorum*, they were supposed to strive to uphold. Instead, with John Duns Scotus and others, they conclude that matter is a partial, incomplete substance that could be conserved by God without any substantial form. When a substance comes into being, prime matter is joined with another partial substance, its form, rather than being caused to exist by the form actualizing it. Neither the Coimbrans nor Suárez go so far as to affirm that matter ever naturally exists without form, though Suárez concludes that, logically, there is no proof to rule this out.[43] Zabarella and our Jesuits thus remain committed to Aristotle's account of substantial change, broadly speaking, despite stark differences in the questions they raise and in how they understand the nature of substantial change, prime matter, and substantial form. Matters are otherwise with Gorlaeus.

Gorlaeus

Gorlaeus, a Calvinist theology student at the University of Leiden, wrote a series of academic exercises on standard topics that stand free from Aristotle's text, but relate to each other systematically. Two further features of Gorlaeus's text set it apart from the commentaries discussed above. First, Gorlaeus writes as a Protestant and more explicitly supports philosophical conclusions with theological reasons. Second, and more significantly, he argues *against* hylomorphism and substantial change, yielding an atomist natural philosophy. Yet Gorlaeus's matter theory, far from being anti-Aristotelian, is established on basic Aristotelian metaphysical principles. Indeed, the theory of accidents and modes that supports Gorlaeus's atomism is consistent with Aristotelian doctrines defended in a standard Calvinist Scholastic textbook of metaphysics widely adopted in Dutch Calvinist universities. For Gorlaeus, as for others who affirmed Calvin's doctrine of the Eucharist, quantitative features were inseparable modes of body, not real accidents.[44] Bodies are in essence *quanta*, and there is only a conceptual distinction between a body and its extension in length, breadth, and depth.

To gain a sense of Gorlaeus's line of reasoning, consider his argument against substantial change, which grounds his argument for atomism. In his first few Exercises, Gorlaeus defends fundamental Aristotelian doctrines of

[43] Suárez (2000, 107). [44] Hattab (2009, 166–172); Gellera (2013).

essence and unity: (1) A real being is that of which the essence exists *per se*; and (2) Every real being is a numerical unity that is distinct from other beings. Gorlaeus further tacitly holds: (3) Real versus fictional wholes must be made of parts that are real beings. From (1) it follows that a real thing is distinguished from others by the essence that makes it what it is. What has its own essence is unable to become the same as something with a different essence except by essential transmutation; i.e., by exchanging its essence for the essence of the other. Furthermore, given (2) and (3), if substances are unions of matter and form, then these parts are undivided in themselves, and divisible from another, the same as with real beings. But each part of a substantial union (prime matter and substantial form) has its own essence. Therefore, these parts are unable to become the same except through essential transmutation.[45] Since the Peripatetics deny that the parts of an individual substance are essentially transmuted when they are united into a whole, they must deny that the resulting entity is a numerical unity. Rather it is an aggregate of real, distinct, indivisible parts.

Next Gorlaeus supports the claim that substantial bodies would not be real entities by aggregation if they did not have parts that were *per se* real beings. Since bodies are not beings of reason, but rather accidental entities by aggregation, they must have real, and hence unitary, indivisible parts. As Gorlaeus puts it, whatever is real is indivisible. He defines division as the mutual segregation of things from each other, and then argues that division consists merely in things separating in place, with both divided things persisting. If division, defined as mutual segregation, could reduce a *res* – a thing – to nothing, then conversely, creation would be composition (i.e., the joining of two things) rather than creation *ex nihilo*. Since this is absurd, division only separates something in place from the thing to which it was previously united, rather than resulting in the non-existence of one or both of the separated things.[46] Natural generation and corruption thus consist, for Gorlaeus, in material atoms being united and separated. The wholes formed by the uniting of atoms are thus not essential affections of being, but accidental beings or rather, coexistences. There is no such thing as substantial change – all natural change is accidental.

Gorlaeus's strict commitment to the Aristotelian understanding of real being, unity, composite substance, and essence puts pressure on the equally Aristotelian view that primary substances are substantial unions of matter

[45] Gorlaeus (1620, 228). [46] Gorlaeus (1620, 236–237).

and form. He bites the bullet and embraces the logical consequences, rejecting hylomorphism and reasoning his way toward atomism.

Conclusion

Older studies of the Scientific Revolution assumed that the new sciences arose in contrast and opposition to a unified Aristotelian orthodoxy sublimated to the demands of religious doctrine. The above examples of Renaissance Aristotelian treatments of substantial change illustrate that the interpretation and application of fundamental Aristotelian metaphysical principles to natural philosophical questions varied greatly from one context to another. Contrary to common supposition, it is not Zabarella's naturalist, Averroist approach that posed the greatest threat to the survival of Aristotelian substance theory. The more theologically inclined Jesuit textbook writers were more innovative, transforming Aristotelian substance theory from within. Finally, Gorlaeus, a Protestant author who was the most explicitly theological, illustrates that modern matter theories did not arise solely from anti-Aristotelian commitments. Some, like Gorlaeus, justified alternative physical principles, such as atoms, by starting from the very foundations of Aristotelian metaphysics, while considering such alternative principles more consistent with God's creation. Even if it is too soon to draw a general characterization of Renaissance Aristotelianism, we can conclude that it was neither uniform nor quiescent, and that the Scientific Revolution owed more to the Aristotelians in its midst than has been recognized.

4

What To Do With the Mechanical Philosophy?

SOPHIE ROUX

A Problematic Category

The mechanical philosophy that emerged during the Scientific Revolution can be defined *prima facie* as a reductionism according to which all physical phenomena are to be explained in terms of corpuscles of different sizes, shapes, and motions.[1] It provided early modern natural philosophers with a unified view of nature that contrasted primarily with the Aristotelian view of nature, but also with other naturalist, hermetic, mystic, occultist, Paracelsian, and chymical accounts. Indeed, early modern natural philosophers devised mechanical explanations of almost every kind of phenomenon, such as gravity, magnetism, chemical operations, the fermentation of grapes, the colors of the rainbow, the circulation of the blood, the motion of the heart, and the development of animals. Even supranatural phenomena, such as transubstantiation, or phenomena today described as paranormal, like the action of sympathetic powder on a wound, the influence of the stars on terrestrial events, or the identification of thieves with a divining rod, were given mechanical explanations.[2]

Confronting this explanatory deluge, historians adopted two opposing narratives.[3] In "The Establishment of the Mechanical Philosophy," Marie Boas characterized early modern mechanical philosophy as "putting the atom to work."[4] According to her, Francis Bacon, René Descartes, and Galileo Galilei resurrected ancient atomism; Robert Boyle grafted the experimental method onto their ontological program; and Isaac Newton articulated the achievements of his predecessors into mathematical laws. In Boas's continuous narrative, the ontology of matter and motion is congenial to

[1] In this chapter, I draw from Roux (1996); Roux (2004); Roux (2013c); Roux (2017).
[2] Digby (1658); Gadroys (1671); Vallemont (1693); Armogathe (1977); Nadler (1988); Del Prete (2001b); Vermeir (2011).
[3] Roux (2017, 31–33). [4] Boas (1952, 520).

experimental investigation of nature and to a mathematized exposition of knowledge. Yet, two years before Boas's article, Eduard Jan Dijksterhuis had concluded that the key factor was not the ontology of machines, but the mathematized science of motion, so that, contrary to the title of his book, one should speak of a "mathematization" rather than of a "mechanization" of the world picture.[5] Richard Westfall similarly contrasted the corpuscular tradition, in which nature is conceived as a huge machine, with the Platonic–Pythagorean tradition, which flourished in mechanics and other mathematized sciences.[6] More bluntly than Dijksterhuis, he castigated Descartes and his corpuscularian fellows for indulging in the construction of imaginary mechanisms and for perpetuating obscure doctrines in domains like chemistry or medicine.

Surely there was an evolution from Boas's continuous narrative to Westfall's point that Newton had to resolve a tension between two conflicting traditions. Except in Boas's paper, the mechanical philosophy was never used by historians of the early modern period without qualification. Nevertheless, whether it was regarded as the ontological foundation of the Scientific Revolution or as a tradition that had to be surmounted, it was viewed as a well-identified category that captured something about early modern natural philosophy. The critical narratives of the last thirty years, however, have deconstructed both the belief that it is an adequate historical category and the conviction that it made a positive contribution to the sciences.

First, in an era that privileged microscopic case studies over sweeping panoramas, and emic over etic categories, 'mechanical philosophy' came to be thought of as lacking any real descriptive significance. It was pointed out that early modern natural philosophers took such complex positions that the frontiers between mechanical philosophers, Aristotelians, and chymists are difficult to draw in practice. Defenders of *minima naturalia* were Aristotelian corpuscularians; conceptual intersections developed between Aristotelians and followers of chymical doctrines; historical links existed between chymists and corpuscular philosophers.[7] The category was also suspected of being unable to account for the various positions actually defended by so-called mechanical philosophers. Helen Hattab, refusing to take Boyle's "rather unique amalgam" as exemplary, insisted on their differences: Descartes was engaged in a foundationalist quest for metaphysical certitude, while Marin

[5] Dijksterhuis (1961, 4, 495–500). [6] Westfall (1977, 1, 36, 42, 120).
[7] Newman (1996); Clericuzio (2000); Murdoch (2001); Newman (2001); Newman (2006).

Mersenne developed a mitigated skepticism; Pierre-Sylvain Régis considered animals as pure machines, while Claude Perrault attributed to them a directing soul; Boyle was optimistic that sensible qualities could be reduced to mechanical affections, while John Locke's epistemology precluded such a reduction.[8] Daniel Garber argued that, while the category of the mechanical philosophy unified Boyle and his contemporaries, this was not so for the *novatores* at the beginning of the century.[9] Lastly, the conceptual identity of the mechanical philosophy began to be blurred. Should it be defined only by corpuscular reductionism, as at the beginning of this chapter? Or should we insist on the elimination of life and soul? On the abolition of the boundaries between artificial and natural beings? On the impossibility of action at a distance? On the rejection of final causes? On the distinction between primary and secondary qualities? Or, still, rather than concentrating on ontological constraints, should we follow an epistemological thread and focus on the claim of the mechanical philosophers that their explanations are clear and intelligible, while Scholastic explanations are obscure and confused?[10]

Second, from an evaluative point of view, it has been claimed that the mechanical philosophy had no impact on the development of the new sciences. As noted, Westfall doubted its value outside of mechanics. Putting some historical flesh on Gaston Bachelard's claim that Descartes should be left in historical isolation because there are neither measures nor equations in his physics,[11] Yves Gingras argued that Newton transformed the notion of explanation by considering that mathematizing a phenomenon was sufficient to explain it. Far from reinforcing mechanization, as Dijksterhuis claimed, mathematization effaced the substances appearing in mechanical explanations.[12] Similarly, with regard to the association between mechanical philosophy and experimental practices, Antonio Clericuzio took issue with the idea that Boyle subordinated chemistry to mechanical philosophy, arguing that his corpuscles were endowed with chemical, not mechanical properties.[13] Contrary to Boas, Alan Chalmers moreover described the link between mechanical philosophy and experimentation as a union *against* nature. Boyle's experimental successes were "achieved in spite, rather than

[8] Hattab (2011a). [9] Garber (2013c). See also below.
[10] McGuire (1972, 523); Pyle (1995, 506–508); Roux (1996, 32–33); Gabbey (2001); Gabbey (2004, 15).
[11] Bachelard (1951, 35).
[12] Gingras (2001); Gingras (2002). See also Gaukroger (2006, 402–403).
[13] Clericuzio (1990). See also Clericuzio (2000, 103–148).

because, of his allegiance to mechanical philosophy."[14] In a word, the mechanical philosophy was said to have – at best – contextual value as a rhetorical meta-discourse designed to legitimize scientific practices.[15]

Considering these descriptive and evaluative challenges, taking the mechanical philosophy at face value is historically inadequate and philosophically naïve. Hence the questions addressed in this chapter: What to do with the "mechanical philosophy"? Does it have to be dropped completely as a category? And, if not, how is it to be used? My view is that it captures something of the transformations that we put under the umbrella of the Scientific Revolution, but that it is not possible to treat it as a naturally given category. Rather, to be employed usefully, it needs conceptual clarification and historical specification.

Uses of the Term 'Mechanical Philosophy'

One difficulty with the concept is the variation in the use of 'mechanical philosophy' across both time and place. *Some Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy*, which Boyle wrote in the late 1650s and published in *Certain Physiological Essays* (1661), is a usual place to begin. In the Preface, Boyle insisted that, notwithstanding the differences between their philosophies, the Cartesian and the atomist hypotheses "might be look'd upon as one Philosophy" because both endeavor "to explicate things intelligibly" and to "deduc[e] all the Phenomena of Nature from Matter and local Motion." At this point, he needed a term to baptize the philosophy that Descartes and Epicurus shared. He actually proposed several names: 'Corpuscular' because it explains natural phenomena through corpuscles; 'Mechanical' because it gives an account of phenomena by motion, which is "obvious and very powerfull in Mechanical Engines"; or 'Phoenician' because its first promoter was the Phoenician Moschus.[16] In one of his last books, *The Christian Virtuoso* (1690), Boyle enumerated the many adjectives used to designate the philosophy of the modern *virtuosi*: it was called the "New," the "Corpuscularian," the "Real," the "Atomical," the "Cartesian," or the "Mechanical" philosophy.[17] As for him, his most common adjectives

[14] Chalmers (1993, 541). Chalmers (2002, 192) grants to Pyle (2002) that he did not prove that the mechanical philosophy obstructed experimental practices and retreats to the weaker thesis that the first did not promote the second.

[15] Gabbey (1985, 13–14); Clarke (1989, 189–190); Gabbey (1990a, 279); Gaukroger (2006, 253–255, 260, 397–406).

[16] Boyle (1999–2000, 2:87), discussed in Roux (1996, 18–20); Gabbey (2004, 16–17); Garber (2013c, 6–8). See also Boyle (1999–2000, 2:260; 5:302; 6:267; 6:455).

[17] Boyle (1999–2000, 11:292).

were 'corpuscular' (or 'corpuscularian') and 'mechanical', which he often presented as synonymous, though there is a nuance between them, the former concentrating on the entities that appear in explanations, the latter on the activities that these entities are engaged in.[18] And to 'philosophy', he obviously preferred 'hypothesis', to emphasize that it was not established once and for all, but only the best hypothesis that was then available.

From a purely terminological point of view, however, *Some Specimens* was not the very beginning. The term 'mechanical philosophy' was used before 1660, initially as a pejorative. After he read *Meteors* and *Dioptrics*, Libert Froidmont complained that Descartes's composition of Aristotelian elements from small parts of various shapes was "too gross and mechanical [*nimis crassa et mechanica*]."[19] For the Aristotelian Froidmont, calling a natural philosophy 'mechanical' revealed the category mistake Descartes had committed: natural philosophy deals with natural beings, so it has nothing to do with artificial things like machines, which were the domain of a mixed mathematical science. The characterization also invited contempt for Descartes's philosophy, just as one might disdain coarse and brutish mechanics working with their hands. A few years later, Henry More also identified the "mechanical philosophy" in order to reject it, dubbing Descartes "the great Master of this Mechanicall Hypothesis" and "pretender to Mechanick Philosophy."[20] Unabashed, Descartes adopted 'mechanical' as a badge of honor and claimed that it was only in as much that his philosophy deals with figures, magnitudes, and motions that it is certain, because this amounts to "a kind of philosophy, where no reasoning is used which is not mathematical and evident, and the conclusions of which are all supported by very certain experiences." Rejecting the Aristotelian distinction, Descartes asserted the equivalence of physics and mechanics,[21] and claimed that "my bloated and mechanical philosophy [*pinguiscula & mechanica philosophia mea*]" could solve many more problems than other natural philosophies.[22]

[18] The book Henry Oldenburg publicized in the *Philosophical Transactions* as "a kind of *Introduction* to the Principles of the *Mechanical Philosophy*" (1666, p. 191) bears the title *The Origin of Forms and Qualities according to the Corpuscular Philosophy* on its front cover and "The Origin of Forms and Qualities according to the Corpuscular *or Mechanical* Philosophy" on its first page. 'Corpuscular(ian)' and 'mechanical' are used as synonymous in Boyle (1999–2000, 5:288; 8:32, 89; 9:38, 286). See also Cudworth (1678, 10–11, 25, 27, 39, 48, 51–52).

[19] Descartes (1964–1974, 1:402–406).

[20] Descartes (1964–1974, 11:243). More (1662, 44) and More (1659) discussed in Gabbey (1982, 220–222).

[21] Descartes (1964–1974, 1:524; 2:212, 542).

[22] Descartes (1964–1974, 1:420–421, 430), discussed in Roux (1996, 15–17); Gabbey (2004, 12–20); Roux (2004, 32–34).

That there were some polemical uses of 'mechanical philosophy' before Boyle does not mean that he did not play an important role. Partly because of his prominence and partly because the program of explaining phenomena in terms of matter and motion was already established in England, the terms 'corpuscular philosophy' and 'mechanical philosophy' were quite successful there, even among the adversaries of this philosophy. In his obstinate crusade against the "pure" and "mere" mechanical philosophy, More actually contributed to the term's popularity as much as Boyle.[23] The adjectives 'mechanical' and 'experimental' were regularly paired together, as if each of them helped make the meaning of the other more precise. Henry Power, for example, opposed "the old Dogmatists and Notional Speculators" to "the Experimental and Mechanical philosopher," who is able "to find the various turnings, and mysterious process of this divine Art, in the management of this great Machine of the World."[24] Likewise, Robert Hooke contrasted "the Philosophy of discourse and disputations" with "the *real*, the *mechanical*, the *experimental Philosophy*," and Samuel Parker explained that the mechanical and experimental philosophy was to be preferred to the Aristotelian philosophy.[25] The association also appeared in attacks on the Royal Society's *virtuosi*, such as those by Margaret Cavendish and Henry Stubbe.[26]

This link between 'mechanical' and 'experimental' might be puzzling. For the modern reader, these terms are not directly related, the first referring to a theory of matter; the second, to a method of inquiring. It is all the more intriguing that 'corpuscular' and 'atomical' were associated with 'mechanical', as in Boyle, but (to the best of my knowledge) never paired with 'experimental'. The solution comes from the polysemy of the English word 'mechanical' in the period. It referred not only to machines and to the science of mechanics, but also to what is related to manual labor, practical skills, artisanal instruments, and concrete operations.[27] The experimental philosophy is "mechanical" in this sense.[28] But there was no similar reason for the experimental philosophy to be called 'corpuscular'.

Continental terminology was quite different. Even those who thought that all natural phenomena have to be explained through matter and motion did not use 'mechanical philosophy' frequently, and, when they did, they did not

[23] More (1659, 11–13); More (1671, sig. B1^{v} and 223, 301, 307–308); Gabbey (1990b, 26–27).
[24] Power (1664, 193).
[25] Hooke (1665, sig. a2^{r}); Parker (1666, 41, 45). See also Hooke (1674, 16).
[26] Stubbe (1670, 4, 6); Cavendish (2001, 49, 74, 93).
[27] Roux (1996, 7–8); Gabbey (2004, 12–14); Roux (2004, 31–33). See also Bennett (1986).
[28] Sprat (1667, 336, 340).

associate it with experiments.[29] Gottfried Wilhelm Leibniz's writings also testify that the use of 'mechanical philosophy' on the Continent resulted from the impact of the Royal Society. When presenting the program of explaining all physical phenomena through corpuscles of different sizes, figures, and motions, the young Leibniz generally attributed it to "the moderns [*recentiores*]," "the new philosophers [*novatores*]," or the partisans of a "reformed" or "restored" philosophy, conceived as a return to Aristotle.[30] Only in reference to the English *virtuosi* does he speak of "corpuscularians" and of "corpuscular philosophy."[31] From the end of the 1670s, Leibniz continued to use 'corpuscular philosophy' occasionally,[32] but more frequent were 'mechanical philosophy' and 'mechanism', which he understood as a general view of nature.[33] Even if he still occasionally interchanged 'mechanical' with 'corpuscular',[34] he more regularly associated the former term with mechanics, understood as the geometrical science that formulates laws of motion.[35] Thus, 'mechanical' is not associated with 'experimental', but connotes necessary chains of mathematical demonstrations.[36]

The semantic connotations are similar in Bernard Le Bovier de Fontenelle. At the beginning of his *Entretiens sur la pluralité des mondes* (1686), he imagined the philosopher as an engineer (*machiniste*) who attends a play and tries to figure out how the spectacle is produced. Phaeton rises into the air; ancient philosophers venture ridiculous explanations, but the Moderns use the principle that a body cannot move except by contact with another body. This prompted one of the rare uses of 'mechanical philosophy' in French, where one can still hear an echo of Froidmont's scorn: "I perceive, *said the Marchioness*, Philosophy is now become very mechanical. So mechanical, Madam, *said I*, that I fear we shall quickly be asham'd of it."[37] Here, 'mechanical philosophy' refers to explanations that compare natural phenomena to machines and are intelligible because they involve only contact actions. The association of mechanical and intelligible also colors the eulogies Fontenelle wrote as secretary of the Académie des sciences. Defending the corpuscular philosophy against Martino Poli's objections, he defined it as the one "where only clear ideas are admitted, that is figures and motions."[38]

[29] See, e.g., Vanzo (2016); Vanzo (2017).
[30] Leibniz (1923–, 2-1:1, 18–19, 25–26, 29–30, 33–34, 266).
[31] Leibniz (1923–, 6-1:489–490; 6-2:325, 327).
[32] Leibniz (1923–, 2-2:396, 845; 6-4:477).
[33] Leibniz (1923–, 2-2:172; 6-4:485, 1559, 1566, 2009, 2118, 2342).
[34] Leibniz (1923–, 2-2:122, 343, 396; 2-3:100; 6-2:325; 6-4:722, 2039).
[35] Leibniz (1923–, 2-1:789; 2-2:82–83, 90, 318, 343, 396, 816; 6-4).
[36] See also Hoffmann (1699, sig. B2^{r-v}).
[37] Fontenelle (1737, 7–10).
[38] Fontenelle (1740, 1:321–322).

Fontenelle also praised Domenico Guglielmini for having undertaken "to reduce this mysterious chemistry to the simple corpuscular mechanics," approving in particular his attempt to explain the properties of salts "geometrically and mechanically."[39]

This terminological enquiry showed how the term 'mechanical philosophy' – an oxymoron for Aristotelians – became a common banner under which early modern philosophers rallied, especially in England, where it was joined to experimental philosophy. It also shows that those who wrote in French were more cautious in their use of the terms 'mechanical philosophy' and 'corpuscular philosophy', and associated them with the necessity and intelligibility of mathematics. At this point, we are confronted with a classical problem for the intellectual historian; namely, the relation between emic and etic categories – i.e., the words used by those who are studied and the words used by scholars to study them. Should we restrict our enquiry to authors who explicitly designated themselves as mechanical philosophers? Or should we admit that there were mechanical philosophers who did not use the term?

Inclining to the former view, Daniel Garber thinks that even if Thomas Hobbes, Pierre Gassendi, and Descartes subscribed to the mechanical view, they are better described as *novatores* who were competing in a race for a new natural philosophy that would supersede Aristotle's; and it was only once Boyle had popularized the term 'mechanical philosophy' that natural philosophers were able to adopt a common paradigm.[40] Garber indeed captures something of the difference between the early seventeenth century, when each philosopher criticized the dominant Aristotelianism in his own name, and the late seventeenth century, when collective institutions stabilized the main tenets of the new philosophy. But no paradigm has ever perfectly unified philosophers. Even after 1660, natural philosophers – mechanical philosophers among them – disagreed about most issues. Moreover, since the term 'mechanical philosophy' was far less used on the Continent though mechanical philosophers were no less common, the presence of the former is not a reliable indicator of the latter.

Last but not least, it is important not to confuse the use of words with the things they refer to. As he himself acknowledged, Boyle did not create the mechanical philosophy out of nothing; there had been already many attempts to formulate a natural philosophy in terms of matter and motion. While recognizing that the popularization of the term 'mechanical philosophy' is

[39] Fontenelle (1740, 1:250, 252).
[40] Garber (2013c). See also his chapter 2 in this volume and Gabbey (2004, 14–21).

historically significant, one should resist terminological fetishism and insist that it captures already existing elements, without which there would have been nothing to name. This is why in what follows, the term 'mechanical philosophy' is used as an etic category – it is applied not only to those who described themselves as mechanical philosophers, but extended to anyone who tried to reduce physical phenomena to corpuscles of different sizes, shapes, and motions.

Articulating the Variety of Mechanical Philosophies

Once the category of the mechanical philosophy has been thus extended, the historian encounters yet another problem, which is that it now seems much too broad to be helpful. Must one examine each early modern "mechanical philosophy" individually? That would be tedious, but it would also miss an essential feature: "mechanical philosophy" was a polemical category put forward *against* other natural philosophies. This observation offers a means for introducing conceptual distinctions in the *mare magnum* of mechanical philosophies. Controversies made explicit what mechanical philosophers expected from their own explanations, and their rejection of alternatives makes their variety manifest.[41] In what follows, I study three such controversies to identify various kinds of mechanical philosophy, or if one prefers, various uses of 'mechanical philosophy': the controversy about the motion of the heart (Descartes versus Vopiscus Fortunatus Plempius); the controversy about the elasticity of the air (Boyle versus More); and the controversy about the universal attraction of bodies (Christiaan Huygens and Leibniz versus Newton).

Descartes versus Plempius About the Motion of the Heart

Recent studies have recognized the importance of his natural philosophy for Descartes,[42] and his project is often described as aiming at reducing all physical phenomena to corpuscles moving according to his three laws of motion.[43] Yet in his exchanges on the circulation of the blood and motion of the heart, he did not claim that his explanations could be deduced from his

[41] For another categorization insisting on the distinction between the corpuscular philosophers, who reduced physical phenomena to corpuscles endowed with various qualities, and the mechanical philosophers, who attributed to them only geometrical properties, so that Boyle is a corpuscular philosopher without being a mechanical philosopher, see Clericuzio (1990); Clericuzio (2000).

[42] Clarke (1982); Garber (1992); Gaukroger (1997); Gaukroger, et al. (2000); Ariew (2011).

[43] Clarke (1982, 122–125); Gaukroger (1997, 94–96, 171–173).

laws of motion. Rather, he contrasted them with explanations that use obscure faculties and insisted that they display necessary sequences of motions.

In all of his writings, Descartes praised William Harvey for the discovery of the circulation of the blood.[44] Yet, from his early treatise *De l'homme* (1633, published posthumously in 1664), he was adamant that, contrary to what Harvey pretended, the motion of the heart had to be explained by a sequence of motions provoked by the extreme heat of the heart, which he conceived as "a fire without light," similar to fires kindled in inanimate bodies.[45] Specifically, the beating of the heart and arteries arises from the expansion of blood by the innate fire as it passes through the cardiac chambers and into the vessels. Descartes insisted that this sequence of motions follows as necessarily from the heat and from the disposition of organs as the motion of a clock would follow from the force and configuration of its counterweights and wheels.[46] While defending his "bloated and mechanical philosophy" against Froidmont, he asked Fortunatus Plempius – professor of medicine at Leuven – what he thought of his explanation.[47] The ensuing correspondence led him to make explicit that explaining the functions of the body requires "no vegetative or sensitive soul or any other principle of motion and life."[48]

In his answers to Descartes, Plempius quickly came to concede that he could approve of the circulation of the blood.[49] He maintained however that physicians had good reasons to prefer Galen's (and Harvey's) view that the heart is moved by a vital faculty. Among these, two prompted interesting replies from Descartes. First, Plempius wrote, the heart goes on beating when it is removed from the body and cut into pieces, and no blood enters or exits it. Second, as Galen demonstrated, if a tube is introduced into an artery, the artery does not beat below the tube, although blood still passes through it.[50]

Descartes suggested different hypotheses to explain how the bits of an eviscerated heart go on beating – some blood might remain, which could be sufficient to make the tissue dilate, all the more if some ferment lies in its

[44] Descartes (1964–1974, 6:50–51; 11:239–240). See Descartes (1964–1974, 4:4, 189, 700). On what follows, see Gilson (1951); Bitbol-Hespériès (1990); Grene (1993); Petrescu (2013); Hutchins (2015); Ragland (2016).
[45] Descartes (1964–1974, 6:46; 11:123–124, 407). See also Descartes (1964–1974, 4:189, 686).
[46] Descartes (1964–1974, 6:50, 11:202).
[47] Descartes (1964–1974, 1:411, 477).
[48] Descartes (1964–1974, 11:208).
[49] Descartes (1964–1974, 2:54). On Plempius's conversion, see Petrescu (2013, 407–410).
[50] Descartes (1964–1974, 1:497–498).

folds. Turning the tables, he added that Plempius's objection is stronger against those who believe that a faculty of the soul makes the heart beat. According to them, the rational soul is indivisible and distinct from the vegetative and sensitive souls, so it cannot cause pieces of the heart separated from the body to beat.[51] In reply, Plempius suggested that the common doctrine could be saved by saying that it is not the rational soul itself that resides in the bits of heart, but its instrument – that is, a spirit that acts according to the power of the soul even if the soul itself is not around.[52] This prompted Descartes to point out that, since Plempius is ready to recognize that these spirits can act even in the absence of the soul – when one is dead – he should recognize that they do not need it even when it is present – when one is alive.[53] In a word, if a physiological phenomenon can be explained without resorting to the soul, then the soul is superfluous. This is an appeal to ontological parsimony, which Descartes invoked more generally in this period, in two complementary ways: first, once a mechanical explanation is given, other entities are not needed;[54] and second, when several explanations are available, the mechanical one requires the least entities.[55]

To Plempius's second objection, Descartes replies that "the laws of mechanics, that is of [his] physics" could account for Galen's experiment. The blood passing from the channel narrowed by the tube to a wider vessel does not have enough force to make the arteries beat.[56] Plempius's faculty is arguably replaced by the laws of mechanics. Yet it should be noted that there is no corpuscular reduction here, and that the qualitative principle at work – the wider the tube through which a liquid passes, the less force it has – cannot be derived directly from Descartes's fundamental laws of motion. Rather, one must add the given structure (the sequence of two tubes, the first one narrower than the second one) to the laws to explain the phenomenon. This can be generalized to the other passages where Descartes invokes laws of mechanics in a physiological context. His three laws of motion do not explain by themselves the phenomena at hand, but have to be applied to specific structures.[57]

These conclusions are reinforced by an additional criticism of Harvey's account that Descartes later developed. According to Descartes, Harvey wrongly claimed that the dilation of the cavities of the heart leads to the

[51] Descartes (1964–1974, 1:523). Fermentation does not depart from mechanical principles: like dilatation, it results from motion.
[52] Descartes (1964–1974, 2:52). [53] Descartes (1964–1974, 2:65).
[54] *Le monde* (Descartes 1964–1974, 11:7, 26). [55] Descartes (1964–1974, 2:200; 3:215).
[56] Descartes (1964–1974, 1:527). [57] Descartes (1964–1974, 4:5; 6:54).

admission of blood, while their contraction leads to its emission.[58] The argument here is not inspired by ontological parsimony, but by a requirement of intelligibility. The trouble is that, on Harvey's account, the motion of the heart is the cause, not a consequence, of the motion of the blood, so one is obliged to make a faculty account for it, "the nature of which is much more difficult to conceive than everything that he pretends to explain through it."[59] Supposing a faculty of contraction without saying how it works does not explain the contraction, the *explanans* duplicates the *explanandum*. On the contrary, Descartes claims, his own explanation is "deduced" "only from the heat [of the heart] and from the conformation of its cavities," "according to the laws of mechanics," all these things being "truthfully mechanical."[60]

Without detailing this correspondence further, let us conclude that Descartes presented his explanation of the motion of the heart as mechanical because it results from a necessary sequence of motions determined by the structure of the heart and, more generally, by the system of the human body. Such a structural explanation does not result from a reduction to corpuscles, the motion of which would be directly determined by the three laws of nature, but appeals to higher-level entities, like flesh, pores, or fibers, and higher-level operations like the lengthening of the heart that causes the expulsion of blood. One might now ask about the relationship between such a structural explanation and a full-blown reductionist explanation. Most commentators, while acknowledging the distinction, maintain that the former will eventually result in the latter.[61] Others argue that Descartes may have faltered upon reaching the outer branches of his tree of knowledge.[62] However, it is imperative to keep in mind that the distinction between structural and reductionist explanations exists for neither Plempius nor Descartes. The only relevant question for both of them was whether faculties can be dispensed with in favor of motions. Descartes concluded that the answer to this question was affirmative, because of a principle of parsimony and a principle of intelligibility, which were convergent according to him.

Boyle Versus More About the Spring of the Air

As we have already noted, it was important for Boyle to define a program to which a significant number of natural philosophers could subscribe. In *The*

[58] Descartes (1964–1974, 1:527; 4:4; 11:241–243). In this letter to Plempius, systole and diastole seem to be confused.
[59] Descartes (1964–1974, 11:243). [60] Descartes (1964–1974, 4:4–5).
[61] Des Chene (2001, 71–72, 83–89, 154); Roux (2004, 34). [62] Hutchins (2015).

Origin of Forms and Qualities According to the Corpuscular Philosophy (1666), he identified more precisely its fundamental tenets.[63] He held that the physical world is made of corpuscles. When clustered into molecules, the corpuscles gain a posture, order, and situation in relation to one another, which Boyle calls a texture.[64] What humans call the sensible qualities are in reality impressions made on our senses by various textures, and forms are nothing but the names given to aggregates of such sensible qualities. All physical change, then, can be reduced to the modification of these aggregates by the motion of the corpuscles. According to this program, the superiority of the mechanical philosophy comes from its capacity to explain all phenomena, starting from clear and primitive notions, without introducing any superfluous entity.[65]

Just as Descartes did not always provide explanations in terms of corpuscles, Boyle did not always provide explanations in terms of textures. But, unlike Descartes, he contrasted his explanations with explanations that use immaterial principles and insisted that they imply experimental properties. His *New Experiments Physico-Mechanical Touching the Spring of the Air and its Effects* (1660) makes the claim "that there is a Spring, or Elastical power in the Air we live in,"[66] and outlines two mechanical explanations of such a spring. According to the first, the corpuscles of the air have a structure that makes them dilate; according to the second, the agitation of the subtle matter surrounding the corpuscles of the air makes them dilate.[67] Unable to adjudicate between these two explanations, he concluded that his design was "not to assign the adequate cause of the Spring of the Air, but onely to manifest, That the Air has a Spring, and to relate some of its effects."[68] Thus, in 1660, Boyle expected from a mechanical explanation that it hypothesizes which corpuscles were the cause of a phenomenon, but he explicitly refused to assign such an explanation to the spring of the air. His controversy with

[63] Boyle (1999–2000, 5:305–334). See Alexander (1985, 62–80); Anstey (2000b, 41–54); Downing (2002, 340–343): Garber (2013b); Roux (2017, 27–29).

[64] See Alexander (1985, 78–79, 85–87); Anstey (2000b, 48–50); Newman (2006, 180–189).

[65] See Chalmers (1993); Anstey (2000b); Downing (2002, 346–347).

[66] Boyle (1999–2000, 1:165). On what follows, see Gabbey (1982); Shapin and Schaffer (1985, 207–224); Gabbey (1990b); Henry (1990).

[67] In the *New Experiments*, the term 'mechanical' is not associated with explanations; its only occurrences are when Boyle mentions the "testimony of a thousand of Chymical and Mechanical Experiments," when he speaks of "Statical and Mechanical Experiments," and when he notes that the "Mechanicall contrivance" of a device would not make it work without a "Chymicall liquor" (Boyle 1999–2000, 1:256, 263, 288). But Boyle explicitly presented these two explanations as "mechanical" in other places (Boyle 1999–2000, 1:150; 3:161).

[68] Boyle (1999–2000, 1:166).

Henry More led him to insist on other meanings of the word 'mechanical' and to upgrade as mechanical his experiments on the spring of the air.

As he explained in a letter to Boyle, More always believed that the "mechanical way would not hold in all phenomena" and that establishing the explanatory gaps it left open would demonstrate the existence of incorporeal beings.[69] Being convinced that Descartes had given the best possible mechanical accounts of natural phenomena,[70] he held that establishing the incompleteness of Cartesian explanations amounted to demonstrating that the phenomena in question were not mechanically explicable, which in turn was to demonstrate the existence of a Spirit of Nature.[71] The thirty-third of Boyle's *New Experiments Physico-Mechanical*, according to which the suction of the exhausted air pump lifts a weight of one hundred pounds, offered him yet another occasion to prove the existence of the Spirit of Nature. For Boyle, "the most plausible *Mechanical* solution" of this phenomenon was the spring of the air. If such a power existed, More argued, it would press a lump of butter with a force equivalent to a hundred pounds and squeeze the liquid out of it. But this does not occur, so the spring of the air is not a proper *explanans* and Boyle's experiment cannot be accounted for without appealing to an immaterial principle.[72]

In his *Hydrostatical Discourse* (1672), Boyle repeats in reply that his intention was only to account for some phenomena by the weight and spring of the air. But while he had previously set aside mechanical explanation of these principles, he now claimed that he had *already* provided them. He notes that his accounts are "grounded upon the Laws of the Mechanicks" and that Archimedes and Simon Stevin, the discipline's founders, did not enquire into the cause of gravity.

> Since then the assigning of the true cause of Gravity is not required in the Staticks themselves, though one of the principal and most known of the Mechanical Disciplines; Why may not other Propositions and Accounts, that suppose Gravity in the Air . . . be look'd on as Mechanical?[73]

Whereas Descartes referenced the laws of mechanics to *eliminate* obscure faculties, here Boyle uses them to *include* gravity, and elasticity with it, among the "mechanical affections of matter," next to motion, bigness, and shape.[74]

[69] More (1659, 196–204); More (1662, 42–47); Boyle (2001, 4:231–232).
[70] More (1659, 12); More (1671, 119, 223); Gabbey (1990b, 30–31).
[71] More (1659, 196–204); More (1662, 42–47).
[72] More (1662, 45–46). For this argument, see also More (1671, 125–150, esp. 140–141); Gabbey (1990b, 22–25).
[73] Boyle (1999–2000, 7:148).
[74] Boyle (1999–2000, 7:159, 183).

For all that, Boyle does not go as far as including the Spirit of Nature in his natural philosophy; he compares its use by More to the way in which the King of China invoked life to account for the capacity of a watch to mark the hours, while the shapes, sizes, and motions are sufficient.[75] Boyle still wants to eliminate superfluous entities, but he modifies his former notion of a mechanical explanation. It first implied reducing phenomena to textures; it now means resorting to the experimentally established properties of machines.

If Descartes could assume that his structural explanations were in principle ultimately reducible to corpuscular explanations, the gap was greater here. Boyle sought a solution to this problem in his doctrine of intermediate explanations. He distinguished between all-the-way-down explanations, "that are most satisfactory for the Understanding, wherein 'tis shewn how the effect is produc'd by the more primitive and Catholick Affection of Matter, namely bulk, shape and motion," and other explanations that are not inadmissible, even if less satisfactory, "wherein particular effects are deduced from the more obvious and familiar Qualities or states of Bodies." For example, even if the density of materials, the spring of the air, or the purgative virtue of rhubarb are not actually reduced to corpuscles in motion, that gold is denser than mercury explains why it sinks in it; the elasticity of the air explains the return to its original form of a compressed bladder; and the purgative virtue of rhubarb explains medical cures.[76] There are indeed many such explanations relying on principles like "Heat, Cold, Weight, Fluidity, Hardness, Fermentation," "Gravity, Fermentation, Springiness, Magnetism," "the Cosmographical, the Hydrostatical, the Anatomical, the Magnetical, the Chymical, and the other Causes or reasons of Phaenomena" – in a word, on "the most usefull notions we have both in Physics, Mechanicks, Chymistry, and the Medicinal Art."[77] Epicureans and Cartesians, if they refuse such principles, will contribute to the advancement of natural philosophy no more than Aristotelians.[78]

Intermediate explanations make room, beyond the requirement of intelligibility, for experimental properties. All commentators now recognize their importance, but their status with respect to the mechanical philosophy is a bone of contention. Using a contemporary demarcation between science and philosophy, Alan Chalmers presents them as a felicitous departure from a misguided mechanical philosophy, which he defines as the reduction of

[75] Boyle (1999–2000, 7:182–183).

[76] Boyle (1999–2000, 2:21–23); Boyle (2004, 8:165^{r}–166^{r}, 169^{r}–170^{r}; 9:40^{v}–41^{r}). See Laudan (1981); Chalmers (1993); Anstey (2002); Chalmers (2002); Downing (2002, 348–349); Chalmers (2010); Newman (2010); Chalmers (2011); Chalmers (2012).

[77] Boyle (1999–2000, 2:21); Boyle (2004, 9:40^{v}).

[78] Boyle (2004, 8:165^{r}–166^{r}).

phenomena to geometrical corpuscles. Peter Anstey and William Newman, on the other hand, describe them as part of Boyle's mechanical philosophy, because they emphasize his later writings and assume his expectations with regard to the mechanical philosophy never changed.[79] Yet attention to Boyle's controversies yields the moderate conclusion that, while always maintaining a principle of parsimony, he may have had different strategies against different opponents. Against Aristotelians' entities, he insisted that the mechanical philosophy reduces phenomena to underlying textures; against More's Spirit of Nature, he insisted that only experimentally established properties are admitted into the mechanical philosophy.

Huygens and Leibniz Versus Newton

The last controversy on which we will focus is the debate about attraction sparked by Newton's *Principia mathematica philosophiae naturalis* (1687). If Descartes and Boyle used mechanical explanations to eliminate obscure faculties and immaterial principles, Huygens and Leibniz privileged intelligibility at the expense of parsimony in their fight against attraction. More precisely, they reproached Newton for wrongly conflating physics with mathematics, instead of completing mathematical descriptions by mechanical explanations that are intelligible. This in turn led Newton to give priority to what is experimentally established over what is intelligible, and thus to distance himself from the intelligibility requirement, or at least from the idea that mechanical explanations would be what best satisfies it.

Newton himself declared in the Preface to the *Principia* that his main result consists in deriving the forces of gravity from the celestial phenomena and, reciprocally, in deriving the celestial motions from these forces. He then wished that "the same kind of reasoning from mechanical principles" would be extended to all other natural phenomena. 'Mechanical principles' does not refer here to the corpuscular principles of the mechanical philosophy, but to the mathematical science of motions and forces, for which Newton claimed the same exactness as geometry.[80] More generally, he insisted on several occasions that forces of attraction and impulse were considered only from a mathematical perspective, not from a physical one.[81] Resistance to this division of labor between physics and mathematics explains the reaction of

[79] Anstey (2002, 164–165, 169–70); Chalmers (2002, 194–197); Chalmers (2010, 5–7); Newman (2010); Chalmers (2011, 151–152).

[80] On the meaning of 'mechanics' in the *Principia*, see Gabbey (1992); Guicciardini (2007).

[81] See, for example, Definition 8 and Book 1, Section 11, scholium, in Newton (1999, 407, 588–589).

the anonymous reviewer in the *Journal des sçavans*, who lauded the exactness of the mathematical mechanics in the first two books of the *Principia*, but criticized the physics of the third book for relying on the arbitrary hypothesis of universal attraction.[82]

Similarly, the irreducibility of physics to mathematics is at the heart of Huygens's attempts to give a mechanical account for gravity. When he first heard of Newton's book, Huygens's response was mixed.[83] On the one hand, he was convinced that Descartes's explanation of terrestrial gravity by a vortex of celestial matter had serious flaws,[84] and he was glad that Newton had "brushed aside" the Cartesian vortices carrying the planets around the Sun as incompatible with Kepler's laws and with the eccentric motions of the comets.[85] On the other hand, he judged that the principle of attraction was "absurd,"[86] because supposing such a quality inherent in matter departed from "mathematical or mechanical principles."[87] Where Newton, in *Principia*, equated mechanics with the mathematical science of motions and forces, Huygens demanded here that mathematical principles should go no further than what is permitted by mechanical principles that form "the true and sound philosophy."[88] Thus, even if Huygens abandoned Cartesian vortices to explain the motion of the planets around the Sun, he still used a subtle matter to explain gravity.

To "make the eye see an image of gravity," Huygens proposed an experiment. Some wax is placed in a closed jar filled with water, which is then spun, so the wax goes to the periphery. When the jar is stopped, the water goes on turning for a while, but the wax goes immediately to the center, the centrifugal force in the water inducing a centripetal motion of the wax. At this point, there is still a disanalogy with gravity – the wax follows spirals, not straight lines, to the center, and it cannot be excluded that the wax's specific gravity is sufficient to explain its behavior. Huygens thus installed small nets that made the wax follow straight lines toward the center of the jar, and he replaced the wax with a body of the same specific gravity as water.[89] Having set up an experiment that represents the most notable characteristic of terrestrial gravity – its centripetal effect – in terms of a circulating fluid,

[82] De Sallo (1689, 237–238). On what follows, see Aiton (1972); Snelders (1989); Gabbey (1992); Lunteren (2002).
[83] Huygens (1888–1950, 9:168–169, 190).
[84] Huygens (1888–1950, 21:455). For other objections, see Huygens (1888–1950, 19:627).
[85] Huygens (1888–1950, 21:143). See also Huygens (1888–1950, 9:368; 10:385; 19:310).
[86] Huygens (1888–1950, 9:538).
[87] Huygens (1888–1950, 21:474).
[88] Huygens (1888–1950, 21:446).
[89] Huygens (1888–1950, 21:453–454). See also Descartes (1964–1974, 2:593–594).

Huygens examines the conditions the subtle matter and its motion must obey in order for this experiment to be transposable to the case of terrestrial gravity. A first condition is that the motion is circular in all directions. Moreover, the matter has to be sufficiently subtle to penetrate all bodies freely. Finally, the theorems on centrifugal force of the *Horologium oscillatorium* (1673) make it possible to calculate that it moves seventeen times faster than an equatorial point on the Earth.[90] Thus, not only was Huygens's intelligible explanation of gravity presented in an experiment, but the speed of the subtle matter could be calculated thanks to the laws of centrifugal force.

When Huygens rejects Newton's attraction, he puts forward the requirement of intelligibility. He claims that one should avoid "obscure and unheard principles,"[91] and find "an intelligible cause of gravity"[92] that sticks to "the true Philosophy in which one conceives the cause of all natural effects by reasons of mechanics."[93] Though Leibniz's *Tentamen de motuum coelestium causis* (1689) aims to explain the motion of the planets around the Sun, not terrestrial gravity like Huygens, it is motivated by exactly the same intelligibility requirement. His letters to Huygens underline that attraction is "an incorporeal and inexplicable virtue,"[94] though the point is still muffled in his 1693 exchange with Newton.[95]

In the 1710s, however, the confrontation became perfectly clear. Whereas Boyle kept presenting his explanations as "mechanical" and claimed that qualities whose existence could be established experimentally were thereby intelligible, Leibniz and Newton realized that a choice had to be made. They agreed on the dilemma: attraction has an existence which is experimentally established, but it is not intelligible; the subtle matter is not experimentally established, but it provides an intelligible cause of attraction. They disagreed about which choice to make. Leibniz chose the second horn: he refused to admit an "unreasonable occult quality," by which he meant saying that bodies tend toward the center of the Earth "without any mechanism, by a simple primitive quality, or by a law of God, who produces that effect without using any intelligible means."[96] Newton chose the first, and his answers to Leibniz were twofold. First, he pointed out that primitive qualities indispensable to mechanical explanations, such as the force of inertia, extension, duration, and mobility of bodies, would be, by Leibniz's lights, occult qualities.[97] However, Newton's most consistent reply was to advance an alternative definition of 'occult qualities'. They are not qualities whose cause is unknown, but whose

[90] Huygens (1888–1950, 21:454–461). [91] Huygens (1888–1950, 21:445).
[92] Huygens (1888–1950, 19:631, 642; 21:451). [93] Huygens (1888–1950, 19:461).
[94] Huygens (1888–1950, 9:523). [95] Newton (2004, 106–109). [96] Newton (2004, 112).
[97] Newton (2004, 116).

existence is not experimentally attested. For Newton, mechanical explanations, when they are opposed to propositions relying on experimentally established qualities, are fictions of our own devising, mere hypotheses, and vain dreams.[98]

The Evaluation of Mechanical Explanations

The debate about universal attraction is crucial to the negative evaluation of the mechanical philosophy mentioned in the first section of this chapter. While, in retrospect, we laud Descartes's or Boyle's attempts to rid natural philosophy of obscure faculties and immaterial principles, we condemn Huygens's and Leibniz's wish to complete the mathematical description of attraction with a mechanism as a retrograde demand for impossible reductions to corpuscles and their motions. There are two strategies to counter this negative evaluation. The first is to reject anachronistic judgments and show the contextual value of mechanical philosophy against various opponents; this is what was done in the previous section of this chapter. The second strategy consists in forging tools for a more positive evaluation.

To do this, we must first avoid focusing on mechanical explanations that were epistemologically weak. There were without doubt mechanical explanations in chymistry, meteorology, and related disciplines, that presupposed untested correspondence between phenomenal properties and the configuration of corpuscles. Thus, as we have seen, Boyle's favorite explanation of the elasticity of the air consists in comparing its corpuscles to little springs.[99] Similarly, Gassendi explains the fall of heavy bodies by the conjunction of two external forces: the pushing force of the air from above, and the force of some magnetic corpuscles emitted by the Earth that pull bodies down as insensible ropes or hooks would do.[100] Or again, Nicolas Lemery associates acidity with pointed corpuscles that open pores, divide some parts and are deflected by others, and sometimes become blunted and other times break.[101] Once we have identified the reasons why such mechanical explanations are unfounded, there is no point in agonizing about them *ad nauseam*. Rather, we have to leave them aside and examine the cognitive benefits that mechanical philosophers were entitled to expect from their explanations.

[98] Newton (1999, 392, 795–796, 943). [99] Boyle (1999–2000, 1:65).
[100] Gassendi (1658a, 3:489–496). [101] Lemery (1675, 10, 15, 44–45, 57, 90, 108, 142, 155–157).

Second, our concept of science has to be broadened and diversified. However crucial experiments and mathematical formalization may be, even today, science cannot be reduced to these two elements, which, moreover, vary from one science to another. Insofar as the negative estimation of mechanical explanations is grounded in a specific model of the science *par excellence* as a physics relying on mathematical laws, it is useful to take another science as a model. One can think in particular of biology, which still places great emphasis on mechanisms.[102]

In this respect, it is to be noted that a number of early modern mechanical explanations, especially in biology, were not based on corpuscles, but on comparisons with machines, which appear as moving structures. To avoid using the neologism 'machinical', but also to comply with a suggestion of Ernan McMullin, we can call these explanations *structural* and characterize them as those causally explaining the behaviors of complex entities by their structures, that is by the "set[s] of constituent entities or processes and the relationships between them."[103] One example we have already met: Descartes's account of the circulation of the blood, where the sequence of motions involved obviously depends on the bodily structure in which it takes place.[104] There are two reasons why such explanations are numerous in anatomy, physiology, and medicine. First, they are appropriate to account for the internal functions of animals – a machine can be broken down into parts, each of which has a certain function with respect to the whole. Second, they are appropriate to account for the functional identity and functional unity of animals – even if the material parts of a machine are changed, as long as it is able to perform its function, it is one and the same machine.

Once the cognitive advantage of structural explanations has been recognized, particularly when it comes to account for animal organisms, it should be emphasized that they were not essentially distinct from other mechanical explanations. On the one hand, since structures can be seen as combinations of corpuscles, there is a continuity between corpuscular and structural explanations. Thus, Gassendi and Boyle suggested that combinations of corpuscles form what they respectively call *semina rerum* or *molecula* and "textures" or "primary concretions."[105] On the other hand, even if, in these

[102] See Roux (2017); Bertoloni Meli (2019).
[103] McMullin (1978, 139, 145–147). See also Gabbey (1985, 11–12); Gabbey (1990a, 274–286); Gabbey (2001, 453–454).
[104] Similarly, see Perrault's account of muscle contraction (Perrault 1680, 75–77), discussed in Des Chene (2005); Roux (2013c).
[105] Gassendi (1658a, 1:282b, 472a, 335b); Boyle (1999–2000, 5:333–334).

explanations, the explanatory burden lies on structures, they do not exclude laws of nature. As we have indeed seen in the case of Huygens's explanation of gravity, the causal structure that was put in place was accommodated to an experiment and to quantified laws of motion. In fact, whether in Giovanni Alfonso Borelli's *De motu animalium* (1680–1681), in Boyle's *A Free Enquiry into the Vulgarly Received Notion of Nature* (1686), or in Fontenelle's *Entretiens sur la pluralité des mondes* (1686), corpuscular explanations, structural explanations, and nomological explanations were expressed in the same breath. All of them were seen as part of the mechanical philosophy, that should be preferred to other natural philosophies, in the words of Samuel Parker, "not so much because of its so much greater certainty, but because it puts inquisitive men into a method to attain it."[106]

[106] Parker (1666, 45).

5

The Later Sects: Cartesians, Gassendists, Leibnizians, and Newtonians

DELPHINE BELLIS

The transformation of early modern science went beyond particular theories and practices. By 1660, the list of *novatores*[1] had considerably shrunk, and new names had replaced the Italian natural philosophers and Protestant atomists once on it. New schools were loosely organized around these new masters, comprising allied disciples and methodological commitments, while attacking each other along new fronts – Cartesians against Gassendists, Newtonians against Leibnizians. This chapter presents the diverse philosophical camps that arose in the later stages of the Scientific Revolution between 1650 and 1750. The individuals associated with these "sects" did not all faithfully subscribe to perfectly explicit, coherent, and systematic programs. On the contrary, declaring membership of a sect was as often a signal of opposition as of allegiance to a methodology or theory. Being a "Gassendist" could simply mean one was not a "Cartesian." Consequently, a later scholar must enquire after the status of these new sects. How did early modern figures identify themselves or others with this or that philosophical school? Relatedly, how did authors view the benefit or detriment of sectarianism to the pursuit of scientific truth?

Philosophical Sects before 1660

Apart from the philosophical schools of antiquity – Stoics, Aristotelians, Pyrrhonians, Epicureans, etc. – the word 'sect' was also used to identify trends of thought proliferating by the first half of the seventeenth century.[2] Heinrich Alsted, in his *Systema physicae harmonicae* (1612), began by identifying four "primary" sects, namely the Mosaic or Christian, the Hebraic, the Aristotelian, and the chemical sect.[3] Jean Chrysostome Magnen, in his

[1] See Garber in this volume (ch. 2).
[2] See Levitin in this volume (ch. 1).
[3] See Lüthy (2000b).

Democritus reviviscens sive de atomis (1646), listed three types of philosophizing: the Peripatetic sect; new sects led by those who claim philosophical freedom (Sébastien Basson, Tommaso Campanella, Robert Fludd, Oswald Crollius, Johannes Kepler, Galileo Galilei, Christoph Scheiner); and sects aiming to revive an old philosophy (like Magnen with Democritus, Nicolaus Copernicus with Aristarchus, Marsilio Ficino with Plato).[4] Alsted and Magnen both applied the ancient scheme of sects to emerging contemporary trends in order to orient themselves in the new philosophical landscape, to select what was worthy in them, and to reconcile them as much as possible.

Walter Charleton, a follower of Pierre Gassendi who joined the Royal Society in 1662, devoted the first chapter of his *Physiologia Epicuro-Gassendo-Charltoniana* (1654) to an examination of philosophical sects and their disagreements. He noted that the ancient Greek philosophical sects had survived into his own age and new ones had appeared. Charleton classified the various schools into four groups: the "pedants" or "Female Sect," "assertors of philosophical liberty," "renovators," and "eclectics."[5] The pedants "become constant admirers of the first Author, that pleaseth them"; of these, the Aristotelians are most important. Of the assertors of philosophical liberty, who reject authority and confide in their own intellectual abilities, Charleton adopted a list similar to Magnen's; i.e., Tycho Brahe, Kepler, Galileo, Scheiner, Athanasius Kircher, and William Harvey, adding "the Epitome of all, [René] Des Cartes."[6] The renovators are those "who, without either totally neglecting or undervaluing the Inventions and Augmentations of the Modern; addict themselves principally to research the Moniments of the *Ancients*, and dig for truth in the rubbish of the *Grecian Patriarchs*."[7] To this group belong Ficino, Copernicus, Magnen, Marin Mersenne, and Gassendi.

Charleton identified himself, together with Daniel Sennert and Jean Fernel, with the eclectics who:

> adore no Authority, pay a reverend esteem, but no implicite Adherence to Antiquity, nor erect any Fabrick of Natural Science upon Foundations of their own laying: but, reading all with the same constant Indifference, and aequanimity, select out of each of the other sects, whatever of Method, Principles, Positions, Maxims, Examples, &c. seems in their impartial judgements, most consentaneous to *Verity*; and on the contrary, refute, and, as occasion requires, elenchically refute what will not endure the Test of either right *Reason*, or faithful *Experiment*. This Sect we may call ... the

[4] Magnen (1646, 28–29). [5] Charleton (1654, 2–4). [6] Charleton (1654, 3).
[7] Charleton (1654, 3).

> ELECTING, because they cull and select out of all others, what they most approve.[8]

Charleton's approach was indeed less conciliatory than that of Alsted or Magnen. He saw the multiplication of sects not as a sign of intellectual vigor, but as some kind of scourge against which to fight.[9] Truth being singular, the more numerous the sects, the greater the risk of being driven away from it. Consequently, Charleton aimed to select among the philosophical positions what was true or closest to truth.

It is worth noticing that Charleton did not identify himself as belonging to the same school as Gassendi, let alone as being a Gassendist (despite the title of his book). In his view, what made the unity of a sect was not a core of shared doctrines, but rather a similar methodology. Additionally, from the beginning of the chapter to its end, Charleton's notion of sect evolved from a purely philosophical one referring to ancient schools to something more religious. Charleton identified three reasons for the great and unfortunate diversity of sects: "the exceeding Obscurity of Nature, the Dimness and imperfection of our Understanding, [and] the Irregularity of our Curiosity,"[10] the third being ultimately most decisive.[11] In this way, Charleton traced the diversity of sects to a human fault (uncontrolled curiosity) rooted in original sin – Eve's desire to gain knowledge. Trying to overcome the plurality of philosophical sects could therefore be seen as a way to redemption. Thus, Charleton applied the ancient category of sect to newer trends of thought but, in so doing, shifted the meaning of 'sect'.

What these views reveal for the period before about 1660 is twofold: (1) there was indeed a proliferation of (self-)professed schools of thought; and (2) there was also a wish to reduce this proliferation with a view to nearing truth. In the event, the proliferation did not endure. Most declared programs never established themselves as full-fledged sects, insofar as they never managed to gather a sufficient number of disciples who could promote and diffuse their research programs. Since most were unsuccessful, the field was left open for the few surviving ones. Indeed, by the 1650s, the views of two figures emerged as preeminent in natural philosophy: Descartes and Gassendi. As Thomas Lennon has argued, this gave rise, in the second half of the seventeenth century, to a "battle of the Gods and Giants."[12]

[8] Charleton (1654, 4). [9] Charleton (1654, 2). [10] Charleton (1654, 5).
[11] Charleton (1654, 8). [12] Lennon (1993).

Cartesians and Gassendists: the French Context

By the middle of the seventeenth century, small academies (e.g., the Cabinet Dupuy; the Académie Montmor, whose permanent secretary was Gassendi's close friend, Samuel Sorbière; and the Académie Thévenot), especially in Paris, created favorable conditions for the diffusion of Gassendist ideas because of their focus on experiments and non-dogmatic discussions.[13] A few professors in the *collèges* were also receptive to atomist ideas – some, like Casimir of Toulouse, even claiming a Gassendist lineage.[14] The lectures organized by Gilles de Launay from 1656 were intended to support Gassendi's ideas against those of Aristotle and Descartes.[15] Conversely, Jacques Rohault's lectures promoted a form of experimental Cartesian physics,[16] and the French *salons* promoted Cartesian philosophy. Likewise, the French Académie des Sciences (founded 1666) was initially focused on experimental practices with no strong commitment to any philosophical doctrine, and even an anti-sectarian approach. To that extent, it "represents something of a triumph for the spirit of Gassendism,"[17] but by the end of the century it had clearly become Cartesian, especially with members like Bernard Le Bovier de Fontenelle and Nicolas Malebranche.[18]

Why was this so?[19] Was Descartes's natural philosophy better suited to the scientific progress of the second half of the seventeenth century? On the one hand, one might argue that Descartes was a far better mathematician than Gassendi. Nothing comparable to Descartes's establishment of the sine law of refraction can be attributed to Gassendi, even if he was a gifted astronomer and conducted important experiments related to the existence of the vacuum and the principle of inertia. The Cartesian view was also more amenable to the mathematizing tendencies described elsewhere in this volume. On the other hand, the main features of Gassendi's philosophy – empiricism, nominalism (only individuals exist), a form of mitigated skepticism (we cannot know the essences of things), atomism (but atoms are finitely numbered and were created and endowed with motive force by God), and acceptance of the void space distinct from matter[20] – seem to win out by the end of the

[13] Brown (1934, 1–16, 64–160, esp. 137); Lennon (1993, 26–33). [14] Brockliss (1995, 458–461).
[15] Brown (1934, 72). [16] Dobre (2013b); Roux (2013b). [17] Lennon (1993, 31).
[18] Brockliss (1995, 463).
[19] This has been partly documented elsewhere. For the prevailing influence of Descartes's mechanical philosophy over Gassendi's in teaching institutions in France, see Brockliss (1995), who accounts for Descartes's victory by stressing the radicality of Gassendi's philosophy and its larger threat to Aristotelian philosophy.
[20] Bloch (1971); LoLordo (2006); Bellis (2017); Bellis (2018).

seventeenth century. The new science, especially in England, bore these hallmarks, oftentimes, if not always, including acceptance of the void.

Yet the comparison of Gassendi's and Descartes's scientific abilities and influence evades the more salient issue: how the early modern philosophers identified the "Cartesian" and "Gassendist" sects. Whereas it is clear, as will be shown, that there was a Cartesian school that defended a rationalist approach to physics associated with plenum and vortex theories, this can hardly be said about Gassendism. There was no Gassendist school, and if the "sect" was mentioned, it was as a rhetorical device, without reference to a real intellectual, let alone institutionalized, group. As Lennon noticed, "there were no Gassendists and there was no Gassendism as there were Cartesians and Cartesianism," and "it is not clear that anything at all called Gassendism ever existed."[21] The victory of the Cartesians in the "battle of the Gods and Giants" was thus illusory – without genuine competition, they won by default.

The Cartesians

From the 1660s, a Cartesian school appeared, mainly in France, composed of figures like Claude Clerselier, Jacques Rohault, Géraud de Cordemoy, and Pierre-Sylvain Régis.[22] The group coalesced in the face of resistance to Cartesianism, exemplified by the placement, *donec corrigantur*, of Descartes's books on the *Index* of prohibited books in 1663, and the 1671 decrees prohibiting the teaching of Cartesian doctrines in the universities issued by King Louis XIV and the archbishop of Paris, François de Harlay de Champvallon.[23] The disciples labored to make their master's philosophy more accepted, adapting it to new scientific discoveries and extending and correcting it where necessary. Robert Desgabets, for instance, tried to show Descartes's views were compatible with religious doctrines like transubstantiation. Others tried to soften the most speculative metaphysical parts, shifting the epistemological status of some propositions – like the threefold partition of matter – toward a more hypothetical status. Some of the physics was also corrected, such as the rules of collision. These refinements were accompanied by a popularization campaign. Clerselier supervised the publication of Descartes's texts. "Wednesday lectures" for a general audience, including vacuum and lodestone demonstrations, were organized by Rohault. And popularizing texts and textbooks, like Fontenelle's *Entretiens*

[21] Lennon (1993, 3–4). [22] Mouy (1934, 98–217); Clarke (1989).
[23] Schmaltz (2017a, 28–29).

sur la pluralité des mondes (1686) and Rohault's *Traité de physique* (1671), were published. (By contrast, no such campaign was conducted by Gassendi's heirs, apart from the publication of Bernier's *Abrégé*, which was still too long for the *honnête homme*.)

Eventually, Cartesianism took root in the Académie des sciences with Christiaan Huygens, Malebranche, and Fontenelle. Malebranche, by integrating Huygens's contributions to physics,[24] stood for a renewed form of Cartesianism. His most important innovation in physics was his theory that each particle of subtle matter is itself a small vortex, thereby accounting for bodies' solidity.[25] In this way, Malebranche extended the Cartesian vortex theory from the cosmological to the corpuscular level. By the end of the century, a Malebranchian physics of vortexes would be adopted to a large extent by figures like Fontenelle (admitted to the Académie des sciences in 1699) and Joseph Privat de Molières. Malebranche was also Cartesian insofar as he considered that all motion occurs by impulsion, and not by attraction as Isaac Newton would have it.[26] As we shall see, this was important to account for the Académie's resistance to Newtonianism.

While Cartesians organized themselves as a coherent school defending and propagating Descartes's natural philosophy, one can still differentiate among them distinct attitudes toward Descartes. One rather slavish view was uncritically described by Claude Gadrois in the preface to his *Système du monde* (1675): the function of the Cartesian disciple is to discover what Descartes's natural philosophy allows (beyond the master's own discoveries), to explain what could have remained obscure or too complex in his texts, and to order his ideas.[27] Régis, author of a *Système de philosophie* (1690), renamed in the 1691 edition *Cours entier de philosophie ou Système général selon les principes de M. Descartes*, took a different perspective. He emphasized the relative uncertainty of speculative physics – the part of physics concerned with the knowledge of causes.[28] Thus Régis claimed he had adhered faithfully to Descartes's general method and principles, but these might lead to particular explanations different from Descartes's.

While their doctrines aligned with Descartes's in many ways, and they helped define the Cartesianism of the late seventeenth century, Huygens and especially Malebranche further distanced themselves from the master

[24] See *Search after Truth* VI.ii.4 (Malebranche 1997, 459).
[25] See *Search after Truth* VI.ii.9 (Malebranche 1997, 520–526).
[26] *Search after Truth* VI.ii.8 (Malebranche 1997, 499–500).
[27] Gadroys (1675, preface).
[28] Régis (1691, 1:275).

and his disciples. Huygens proposed a different vortex theory to explain gravity. He also thought that the mechanical philosophies of the sectarians of Descartes or Democritus might account for physical phenomena, but not for organic ones.[29] Moreover, Huygens was critical of the Cartesian sect *per se*, and did not identify himself with it. The setting up of the Cartesian school, he wrote, was made possible because Descartes, contrary to Gassendi, wanted to produce an entire system and, contrary to Galileo, had the vanity of becoming a "*chef de secte*."[30] Likewise, Malebranche refused to be enrolled in the Cartesian sect on the grounds that this was contrary to Descartes's philosophy and its rule of evidence.

> It is an unworthy thing to become a partisan of any sect, whatever it may be, and to regard its authors as infallible. Descartes too, wanting to make men disciples of the truth rather than obstinate sectarians of his opinions, expressly warned: that we not place any faith at all in what he wrote, and that we accept from it only what the strength and evidence of reason might constrain us to believe.[31]

This criticism indicates the negative connotation, not so prominent before, that became attached to the notion of 'sect' toward the end of the seventeenth century – a development I will return to below.

The Gassendists?

Despite what I said above on the non-existence of a Gassendist school, strictly speaking, one encounters early modern uses of the term 'Gassendists' as if it corresponded to a school. In his *Voiage du monde de Descartes*, Father Gabriel Daniel listed "the heads of the new sects of philosophy, among which we easily recognize Mr. Descartes, Mr. Gassendi, Father Maignan, and several others."[32] Pierre-Daniel Huet identified Gassendi as the head of a party who renewed Epicurus's sect against Descartes.[33] As for its members, Abbot A. Martin listed François Bernier, David Derodon, Jean-Baptiste Du Hamel, and Edme Mariotte as disciples of Gassendi.[34] But rather than disciples, Gassendi had admirers who appropriated some of his doctrines, but did not defend them *qua* Gassendist. Some were called "Gassendists" because they accepted the existence of atoms and the void, but for all that, they did not always endorse Gassendi's philosophy. For example, despite his listing by

[29] Huygens (1702, 34–35). [30] Huygens (1888–1950, 10:404).
[31] *Search after Truth*, conclusion to books IV–VI (Malebranche 1997, 528–529).
[32] Daniel (1690, 113). [33] Huet (1723, 6). [34] Martin (1854, 277–278).

Martin, David Derodon referred only to Democritus and Epicurus in his 1661 *Dissertatio de atomis*.

The absence of a Gassendist school in natural philosophy is certainly not due to the "libertine" aspect of his philosophy – after all, Gassendi was highly regarded and his morality never contested in the seventeenth century – or his materialist leanings, but maybe more to the antiquarian and anti-dogmatic dimensions of his methodology. Gassendi's anti-dogmatism prevented him from elaborating his natural philosophy as a rigid set of doctrines. Atomism was for him only the most probable matter theory, but not a perfectly certain truth. It also prevented his followers from gathering into a school dedicated to the defense of an orthodox version of their master's doctrines. After all, it was easier to defend a core of well-established doctrines, as in Descartes, than the tentative statements one finds in the *Syntagma philosophicum*.

Those who explicitly aligned themselves with Gassendi did not do it in a servile way and claimed the freedom to distance themselves from him on some points, whereas the Cartesians who modified Descartes's doctrine claimed to express it in a way more faithful to their master's implicit intent. Gilles de Launay is a case in point. He gave popular Gassendist lectures in Paris, published *Les essais physiques* (1667), and was a member of the Académie Bourdelot. He defended the existence of atoms and the void, and followed the structure of the Physics part of the *Syntagma philosophicum* quite closely. Also in keeping with Gassendi's philosophy, he rejected the Cartesians' dogmatism and valued verisimilitude when truth was not accessible.[35] De Launay wrote that

> after having long examined which sect I would most willingly embrace, I resolved to follow the principles of the learned Gassendi, when they are conformable to Christianism and good morals. My design, however, is to attach myself to a particular doctrine of ancient or modern philosophy not because of its authority, but because of the weight and force of its reasons, and for the sincere love I have for embracing the truth, to receive it from the good faith of all sorts of authors, and to seek it by various experiences.[36]

De Launay was faithful to Gassendi in his attachment to this philosophical freedom, including when this led to departure from Gassendi. He attacked the Cartesians by using Gassendist arguments, but was not a systematic Gassendist. De Launay was even ready to accept parts of Aristotle's doctrine

[35] Launay (1667, 12).
[36] Launay (1667, 5). See also Launay (1667, 11–12) about the philosophical freedom he wanted to preserve with regard to Gassendi.

and reconcile his principles with his. Moreover, he did not associate with others in order to constitute a Gassendist school.

'Gassendists', then, did not describe a coherent group. Instead, the category was forged in order to rhetorically organize the distinction between the Cartesian school and those who did not fully adhere to the Cartesian program, even if they did not constitute a unified group with a single research program. This addressed the polemical problem that the distinction was not always readily apparent. Despite their differences, there was a proximity between Descartes's and Gassendi's natural philosophy. They shared the rejection of substantial forms, a particle (corpuscularian or atomist) physics, and the preeminence of efficient cause (i.e., motion) and of extension and shape. In comparison with Aristotelian natural philosophies, they had more in common with each other than with the old view of nature.

There was a consequent tendency by opponents to lump together the Cartesian and "Gassendist" positions, at least insofar as they opposed Aristotelianism and proposed some form of mechanist natural philosophy. Daniel, in his *Voiage*, tended to consider Gassendi as a kind of disciple of Descartes and a member of the Cartesian sect.[37] Yet the same is true of their supporters. The Cartesian Régis, for example, seemed to consider Gassendi and Descartes as equals. This is due to the fact that Régis thought that there was one system inherited from ancient times to which more recent natural philosophers had only added new hypotheses.[38] The primary truths on which, according to Régis, physical hypotheses relied were so general that they could be compatible either with Descartes's or with Gassendi's physics. Meanwhile Charleton, more aligned with Gassendi, enlisted Descartes in the defense of atoms:

> Insomuch therefore, as the Essential reason or Formality of *Corporeity* doth solely consist in *Extensibility*, or the Dimensions of Longitude, Latitude, and Profundity real . . . and insomuch as nothing can be the *Root* or beginning of Material or Physical Extension, but . . . *Aliquid indissolubile*, something so minute and solid . . . such as are the ATOMS of *Democritus*, *Epicurus*, and other their *Sectators*; and the *Insensible Particles* of *Cartesius* . . . .[39]

In their campaign for acceptance, the Cartesians had to distinguish themselves from other forms of natural philosophy, and especially from any seen as close to Epicurus's materialism and atheism.[40] 'Gassendist' was thus employed to primarily refer to (and criticize) those who believed, like

[37] Daniel (1690, 185). [38] Régis (1691, 1:276). [39] Charleton (1654, 85).
[40] See Rohault (1671, 196–203).

Epicurus, in the existence of indivisible atoms and of the void. This was all the more necessary as Cartesian corpuscles looked very much like Gassendi's atoms, despite Descartes's claims to the contrary and his emphasis on the divisibility of any bit of matter. When the Cartesian Cordemoy adopted atoms and the void, he sparked a strong reaction from Desgabets, who criticized Cordemoy's ontology with a view to defending the Cartesian camp.[41] In a letter to Clerselier, Desgabets wrote that Cordemoy was making a schism that "in one blow, takes from true philosophy one of its strongest columns, and significantly fortifies the party of Monsieur Gassendi."[42] By contrast, adopting other features of Gassendi's philosophy did not as readily make one a "Gassendist." Claude Perrault adopted a Cartesian vortex theory to account for weight, but subscribed to Gassendi's empiricist and non-dogmatic epistemology.

'Cartesian' and 'Gassendist' can be considered actors' categories, yet their significance was quite distinct. One was the self-identification of a real school of thought. The other served only to designate its adversaries; either as a means of finding a reconciliation, as in the eclectic program of Charleton, or, on the contrary, as a basis for polemical rejection.

Boyle's Anti-Sectarian Program in Natural Philosophy

As noted above, the polemical use of 'sect' eventually cast the notion in a negative light. Jean-Baptiste de La Grange, echoing Malebranche, disparaged sects as a whole in *Les principes de la philosophie contre les nouveaux philosophes, Descartes, Rohault, Regius, Gassendi, le père Maignan, etc.* (1675).

> I am no longer surprised to read in ecclesiastical history that there have been sects of heretics, who taught opinions meriting more the name of folly and extravagance than that of error and heresy. When men are in the mind to conspire, and attach themselves to someone who dogmatizes, they never fail to enter into his sentiments, even those as absurd as can be.[43]

By contrast, Sorbière, in his *Relation d'un voyage en Angleterre* (1666), described the Royal Society as a place where no single authority prevailed. There was a division, insofar as the mathematicians were more inclined toward Descartes and the literate toward Gassendi,[44] and the Society added its own tutelary figures, like Bacon and later Newton, but this did not immediately

[41] See Ablondi (2005). [42] Prost (1907, 158). [43] La Grange (1675, 344–345).
[44] Sorbière (1666, 76).

devolve into sectarianism. Indeed, one of the Royal Society's aims was to avoid the sectarianism that had caused political troubles in England. The investigation of nature, according to Thomas Sprat, was supposed to allow for disagreement without leading to wars between factions or sects.[45] The founding members of the Royal Society had "attempted to free it from the Artifice, and Humors, and Passions of Sects."[46]

Much of this was due to the influence of Robert Boyle, who tried to overcome the oppositions between different sects.[47] Boyle considered the sectarian attitude based on arguments of authority "borrowed from this or that sect of philosophers" contrary to reason and counterproductive in the search for truth.[48] Natural philosophy itself was considered "a non-sectarian way of understanding God's creation, on a par with scripture."[49] Natural philosophy – especially of an experimental, non- or anti-speculative sort – thus offered a common ground upon which to build a simple natural theology drawn from the book of nature, entirely free from the religious dissensions that had so corrupted the English body politic. As a consequence, Boyle approved an eclectic program, compatible with experimental practices and going beyond sectarian divisions bearing on the metaphysical principles of natural philosophy.

What Daniel Garber labels Boyle's "*irenic* program of the mechanical philosophy"[50] had consequences for the way Boyle interpreted what could be seen as different philosophical sects on the Continent. Boyle evoked "two of the chief sects of the modern philosophizers," namely the followers of Epicurus and those of Descartes, and declared: "I esteem'd that notwithstanding these things wherein the Atomists and the Cartesians differ'd, they might be thought to agree in the main"; their differences "seem to be rather Metaphysical than Physiological Notions."[51] In *The Origin of Forms and Qualities according to the Corpuscular Philosophy*, Boyle attempted to blur the differences between philosophical sects, in particular about the divisibility or indivisibility of elementary particles or the existence of void.[52] As Garber put it, Boyle's anti-sectarian approach "can be read as a call to arms for a new program intended to respond to the chaos of anti-Aristotelian views."[53]

[45] Sprat (1667, 56, 91). [46] Sprat (1667, 62).
[47] See Jacob (1977). For a more nuanced interpretation of Boyle's complex relation to religious sects, see Hunter (2000, 51–57).
[48] Boyle (1772b, 4:179–180). [49] Gaukroger (2006, 149). [50] Garber (2013c, 8).
[51] Boyle (1772b, 2:87). [52] Boyle (1772b, 5:292). [53] Garber (2013c, 22).

Leibnizians and Newtonians

Boyle's call was not always heeded. By the end of the seventeenth century, two new natural philosophies – and their partisans – emerged, radically modifying the philosophical landscape: those of Gottfried Wilhelm Leibniz and Newton. At first, they both challenged Cartesian natural philosophy; then they opposed one another, especially on metaphysical and theological issues.

Leibniz took Cartesian mechanism as a starting point for physics, but held that mechanism was insufficient to account for the laws of nature. Thus, Leibniz rejected the Cartesian identification of body with extension,[54] and reintroduced metaphysical notions like force, substantial forms, and final causes, without simply returning to Scholasticism.[55] Against the Cartesian principle of conservation of quantity of motion, Leibniz formulated the principle of conservation of force and elaborated a proper science of motion and force – a dynamics.[56] Newton also rejected Cartesian philosophy as insufficient. In its place, Newton promoted a new method for science based on experience and mathematization (anticipated to some extent by Huygens) and, against the Cartesian vortex theory, proposed his theory of universal attraction.

There are some grounds for debate about the extent to which these theoretical differences hardened into factional divides. For his own part, Leibniz adopted a conciliatory, anti-sectarian attitude, which converged with his own irenic attempts to overcome religious dissensions. As early as January 1679, Leibniz wrote to Malebranche regarding his *Conversations chrétiennes* that he wished Malebranche had not written only for the Cartesians because it seemed to him that every name of a sect should be unbearable to anyone who looked for the truth. Leibniz also criticized "*l'esprit de secte*" of "the Cartesians" who were "simple paraphrasers of their master."[57]

Also, J. B. Shank has denied that there existed an "intransigent war between Newton and Descartes as the dominant theme of Newton's French reception."[58] On the contrary, "France absorbed Newton's science [i.e. its mathematics, analytical mechanics] immediately and in substantial, if

[54] *Discourse on Metaphysics* §12 (Leibniz 1989b, 44). See Gorham and Slowik in this volume (ch. 22).
[55] *New System of the Nature and Communication of Substances* (Leibniz 1989b, 138–139); *Discourse on Metaphysics* §11 (Leibniz 1989b, 43).
[56] See Stan (ch. 21) and Rey (ch. 24) in this volume.
[57] Leibniz (1923–, 2-2:534).
[58] Shank (2008, 19).

idiosyncratic, ways from as early as 1690."[59] According to Shank, opposition between Cartesians and the Newtonians was "invented" by Fontenelle and Voltaire. The latter, for example, unsuccessfully attempted to convince Pierre Louis de Maupertuis, in a letter of April 29, 1734, to become a Newtonian *"chef de secte."*[60] Fontenelle, in his 1729 *Histoire de l'académie royale des sciences*, presented Privat de Molières as the Cartesian defender of the vortex theories against Newton and his "numerous sect."[61] This suggests that the sectarianism in natural philosophy was only revived in the 1730s.

However, there is support for the contrary view. The relations of the new schools of natural philosophy took a more polemical turn in response to Newton, against whom both the Cartesians and the disciples of Leibniz organized opposition on the Continent. As Mordechai Feingold has suggested, Giovanni Domenico Cassini's commitment to defend the Cartesian heavens against Newton's theory as early as the 1690s is a case in point.[62] Moreover, Carlo Borghero has shown that the opposition to the reception of Newtonian natural philosophy was deeply rooted in Cartesianism in its Malebranchian form, and that it was much more long-lasting than Voltaire's presentation of the success of the Newtonian theories in France in the 1730s might lead us to think. In 1738, for instance, Privat de Molières criticized Newtonianism by claiming for himself a Cartesian mantle. For him, Newton's theories were only acceptable on a purely descriptive, mathematical level, and as far as causality was concerned, mechanism in the form of Malebranche's small vortices prevailed. About his dispute with the Newtonian Pierre Sigorgne, he claimed that if he were successful (as he thought), the Cartesians had won, and the Newtonians should admit it.[63] There is also Jean le Rond D'Alembert's statement, in the 1743 first edition of his *Traité de dynamique*, that the Cartesians of the Académie des sciences still constituted a sect, though weakened by Privat de Molières's death and Jean-Jacques Dortous de Mairan's departure for the Académie Française.

The Newtonians did not face resistance only from the Cartesian camp, but also from the Leibnizian one. Continental Leibnizians (mainly in Basle and Paris) and British Newtonians were first opposed because of the quarrel over priority in the invention of the calculus (mainly launched by the Newtonians).[64] Then two schools developed, cemented by different methodological values and principles. Apart from actively campaigning for the defense of Newton's priority in the calculus dispute, Newton's disciples

[59] Shank (2008, 28). [60] Voltaire (1977, 510). [61] Fontenelle (1731, 87).
[62] Feingold (2010).
[63] Borghero (2011, 118). [64] See Hall (1980).

published books intended to serve as introductions to some parts of Newton's *Principia*; e.g., John Keill's *Introductio ad veram physicam* (1702) and *Introductio ad veram astronomiam* (1718), David Gregory's *Astronomiae physicae & geometricae elementa* (1702), and William Whiston's *Praelectiones physico-mathematica Cantabrigiae in scholis publicis habitae: quibus philosophia illustrissimi Newtoni mathematica explicatius traditur, & facilius demonstratur* (1710). Until about 1750, the British Newtonians (David Gregory, Abraham De Moivre, Roger Cotes, Keill) favored geometrical proofs in their published writings and rejected the use of infinitesimals and uninterpreted symbols.[65]

On the continent, on the contrary, the Leibnizian mathematicians (the Bernoulli brothers Johann and Jacob, Guillaume de L'Hospital, Jakob Hermann, Pierre Varignon) made the calculus public through the *Acta eruditorum* and the *Mémoires* of the Académie des Sciences. Leibniz supported this approach as a way to create a mathematical school able to master his calculus. Convinced of the superiority of the analytical method over Newton's geometrical one, Johann Bernoulli (in a competitive relation to the Newtonians) and Varignon (in a more conciliatory mode) attempted to demonstrate this superiority by applying Leibniz's calculus to Newton's *Principia*.[66] From the beginning of the eighteenth century, Christian Wolff, under Leibniz's supervision, wrote most of the reviews of mathematical books published in the *Acta eruditorum*, which had taken a general anti-Newtonian turn, and was perceived as such among the British.[67]

Then the core of a philosophical debate between Leibnizians and Newtonians, sparked by the correspondence between Leibniz and Samuel Clarke (1715–1716), came to bear on the nature of space, the void, and the role of God in the created world. Against the voluntarist conception of God and the conception of space as absolute and independent of bodies, defended by Newton and Clarke, Leibniz privileged an intellectualist conception of God and considered space to be no more than an order of relations. Following Leibniz, the Bernoullis and Hermann rejected the Newtonian action-at-a-distance in cosmology.

Thus, by the beginning of the eighteenth century, what had started as a priority dispute in mathematics solidified into the opposition of philosophical factions. Leibniz himself denounced what he perceived as "the spirit of factions" within the Royal Society.[68] Later, the opposition between the Leibnizians and the Newtonians manifested at the Berlin Academy of

[65] Guicciardini (1999, 169–194). [66] See Guicciardini (1999, 195–260).
[67] Hall (1980, 164–165). [68] Leibniz (1965, 3:328).

Sciences, where Maupertuis – its president – and Leonhard Euler, two supporters of Newton, opposed Leibniz's and Wolff's metaphysics.[69] A question proposed in 1745 by the Academy could be viewed as an implicit invitation to refute the doctrine of monads and show its uselessness as the foundation for explanations of natural phenomena. Euler, in his *Gedanken von den Elementen der Cörper* (1746), also criticized the metaphysics of monads. The tension between the Leibnizians and the Newtonians at the Academy erupted in a dispute over the principle of least action. Maupertuis, as the discoverer of this principle, held that it had to be based on physical and mathematical proofs, and should not be derived *a priori* from metaphysics. But in 1751, Samuel König published a paper in which he quoted a letter from Leibniz to Jakob Hermann in which König claimed to have found the first formulation of the principle of least action. Maupertuis, supported by Frederick II, perceived this as an accusation of plagiarism and the dispute led the Academy to officially reprimand König in 1752, while Voltaire, who had supported König, left Berlin and went back to France.[70]

Conclusion

With the emergence of the new science and the replacement of Aristotelianism with Cartesianism as the philosophical school thought to provide the foundations for the new science, one can observe, by the mid-seventeenth century, a shift in the use of the category of sect. While it still conveyed the neutral meaning of something like an ancient philosophical school in the eclectic program of an anti-dogmatic like Charleton, it took on a more negative polemical meaning, intended to disparage one's opponents or promote a conciliatory program, such as Boyle's. However, the term was still used in a non-critical way until the beginning of the eighteenth century, less to refer to a core of shared doctrines, and more as a way to build strategic alliances against other philosophical schools that did not share the methodological principles (rejection of metaphysical hypotheses, importance of experiments) perceived as appropriate to support the new science. Despite calls for conciliatory research programs, sectarian attitudes did not disappear by 1750, but delineated new battle lines between the Cartesians, the Leibnizians, and the Newtonians.[71]

[69] See Leduc (2015). [70] Knobloch (1995).

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6

Confessionalization and Natural Philosophy

ANDREAS BLANK

Introduction

This chapter addresses prominent considerations both for and against the *confessionalization thesis* – the view that theological contents specific to the Catholic, Lutheran, and Reformed creeds had an influence on the theoretical contents of early modern natural philosophy. This thesis intersects with the larger question regarding the interrelations between science and religion, a question that has shaped many debates in the past decades. The question regarding this thesis's truth is both narrower and wider than the question concerning the influence of religion on the emergence of modern science. It is narrower because it does not go into the wider issue of whether the Reformation brought about changes in general cultural attitudes toward work ethics, social organization, and profitable education that, in a very indirect way, turned out to be supportive of the development of experimental methods, the mathematization of natural laws, and the search for technological innovation.[1] It also leaves aside the role that institutional changes connected with the Reformation and Counter-Reformation – such as educational reforms and new patterns of network formation – had in the emergence of the sciences.[2] Furthermore, the religious views of some of the most important early modern natural philosophers – e.g., the radical reinterpretation of the doctrine of the Trinity in Henry More, the anti-Trinitarian theology of Isaac Newton, and the acceptance of a conception of an immaterial, divine space in both thinkers – can hardly be understood as outcomes of the homogenizing pressures of confessionalization.[3] But the question

[1] Merton (1936); Webster (1975). For critical discussion, see Mulligan (1980); Cohen, et al. (1990).
[2] See Detlefsen in this volume (ch. 7).
[3] Austin (1970); Coudert (1975); Markley (1989); Snobelen (1999); Force and Popkin (2013); Iliffe (2017).

concerning the influence of confessionalization on natural philosophy is also wider than that regarding the influence of religion on science, since early modern natural philosophy comprised many themes that are not regarded as parts of what came to be called "natural science." In sum, there is an imprecise overlap of "confessionalization" and "religion," on the one hand, and of "natural philosophy" and "science," on the other.

It will thus be useful to begin with some conceptual clarifications. Confessionalization (*Konfessionsbildung*) is a concept developed by historians such as Ernst Walter Zeeden and Wolfgang Reinhard.[4] It is meant to describe a political program pursued by the authorities of the emerging early modern states and the newly founded churches in the Protestant world, as well as the response of the authorities of the Catholic Church and the Catholic territories to this development. This political program involved a variety of elements: One element was the negotiation of agreements or compromises about matters of theological doctrine – a process that, on the side of the Protestants, resulted in a large number of texts that defined what believers were supposed to confess. On the Catholic side, this development was countered by a gradual consolidation of dogma, most prominently during the Council of Trent (1545–1564). Another element of confessionalization went beyond the specifically religious realm and included detailed regulations of social customs and personal habits.[5] A third element, in both Protestant and Catholic contexts, consisted in building up institutions for the close surveillance of religious beliefs and practices in the parishes.[6]

How successful was the political program? If one examines the negotiation protocols and draft versions of texts defining confessional tenets, it becomes clear that the finally accepted versions often papered over deep and unresolved theological disagreements. If one looks into the extant visitation reports, it becomes clear that heterodox beliefs and even remnants of paganism remained pervasive in popular culture. If one considers the ever-increasing penalties inflicted on the ever-same transgressions of the customs and habits regulations, it becomes clear that the population in the sixteenth and early seventeenth centuries was recalcitrant to a degree hardly imaginable in later centuries. Is it plausible, then, to assume that early modern natural philosophers simply succumbed to the homogenizing pressures of confessionalization?

An answer to this question depends partly on how the disciplinary identity of early modern natural philosophy could be described. What seems

[4] Zeeden (1965); Reinhard (1983); Zeeden (1985). [5] Schilling (1994).
[6] Zeeden and Molitor (1977).

uncontroversial is that, due to the persistence of Aristotelian and Neoplatonic modes of thought, natural philosophy in the sixteenth and seventeenth centuries covered the scope of the Aristotelian works devoted to natural particulars – *Physics*, *On the Heavens*, *Meteorology*, *On Generation and Corruption*, *History of Animals*, *On the Parts of Animals*, *On the Generation of Animals*, *On the Soul*, and *Parva naturalia* – as well as themes prominent in Plato's *Timaeus* and its Renaissance readings. Natural philosophy therefore included logic, metaphysics, methodology, cosmology, and philosophical psychology. What is controversial, however, is the question of the relation between early modern natural philosophy and theology. This question has led to a spirited controversy between Andrew Cunningham and Edward Grant. Cunningham observes that there are numerous programmatic passages in the early modern works that describe the point of doing natural philosophy as gaining insight into divine providence.[7] Grant has objected that, for the most part, these programmatic statements have left no traces whatsoever in early modern treatments of the details of natural things, which is why he concludes that these treatments are not at all about God.[8] Luckily, the controversy need not be resolved here, since it raises questions that go far beyond the scope of the present chapter. Evidently, however, the question of the influence of confessionalization on natural philosophy is relevant to this controversy, and, as far as this goes, both positions seem to involve exaggerations.

Some leading historians of science have used aspects of confessionalization to explain specifics of early modern natural philosophy, and it has even been suggested that there was something that could be described as *the* confessionalization of early modern physics.[9] Such claims have triggered weighty criticisms. In what follows, I will sketchily present four case studies that indicate some senses in which the confessionalization thesis seems to be well-founded, as well as some senses in which existing criticisms seem to be persuasive. In addition, I will present some further source materials that point to the conclusion that one ought to be cautious about making too general claims concerning the pervasive role of confessionalization in early modern natural philosophy. Since some of the generalizations found in the literature are mainly (but not only) based on sources from the English-speaking world, I will counterbalance such approaches by considering mainly (but not only) sources from other parts of Europe.

[7] Cunningham (2000). [8] Grant (2007, 251, 301–302). [9] Lüthy (2005).

Confessionalization and Cosmology

The decision of the Catholic Church to commit itself to geocentric cosmology explains why Catholic natural philosophers regarded it as a duty to defend central aspects of Ptolemaic cosmology. Protestant astronomers were more open to explore alternatives. Initially, the truth of Copernican astronomy (even if not its usefulness in making predictions) was contested by Philipp Melanchthon, but through the work of Georg Joachim Rheticus, heliocentric planetary motions soon began to be regarded as a genuine possibility.[10] As Grant has brought to light, the Jesuit defense of the immobility and centrality of the Earth involved a number of innovative considerations, too. For instance, Giovanni Baptista Riccioli abandoned the medieval view that the center of the universe is the most ignoble place. Riccioli argued instead that the center of the universe must be the place of the Earth because the Earth is the place of living beings – that is, the most perfect beings in the created world. Moreover, simplicity considerations played a role – superior celestial bodies are better adapted for circular motion than the Earth due to the heaviness of the elements of earth and water.[11] Perhaps the most innovative suggestion can be found in Jesuit accounts of the nova of 1572 as a cluster of denser ethereal parts that look like a new star.[12] According to such accounts, what takes place in the celestial realm is accidental change, but not substantial change. The Jesuits also developed responses to Tycho Brahe's arguments against hard spheres. They accepted Tycho's observations that show that the orbits of Mars and the Sun intersect and that the movement of the comet of 1577 took place in the celestial region beyond the Moon. Robert Bellarmine and Christopher Clavius suggested that these observations point to the conclusion that heavenly bodies move through a fluid medium,[13] while Roderigo de Arriaga suggested that the relevant medium could be of a nature that lies somewhere in the range between a liquid and a gas.[14]

Protestant astronomers were not doctrinally compelled to find such remedies for a geocentric cosmology. Beyond this lack of theological commitment, some historians have argued that there was a specifically Lutheran astronomy whose general characteristics derive from the view that natural events refer to moral and spiritual truths, so the normal course of nature is revelatory in that it expresses the order in the mind of God.[15] In more technical terms, the

[10] Westman (1975).
[11] Grant (2003, 129–131).
[12] Grant (2003, 136–137).
[13] Lattis (1994).
[14] Grant (2003, 141).
[15] Westman (2011).

distinction between general providence, understood as divine support of the normal course of nature, and special providence, understood as divine intervention in the normal course of nature, became prominent in Lutheran cosmology. For instance, Melanchthon attributes phenomena such as comets to special providence, as signs of the will of God and as warnings against immoral ways of living.[16] Similarly, the Lutheran theologian Jacob Andreae interpreted the nova of 1572 as an instance of special providence,[17] and the astronomer Joachim Camerarius the Elder regarded such singular heavenly phenomena as announcements of disasters.[18] The belief in singular divine signs in nature was the theoretical foundation for the defense of astrological beliefs. For instance, the astronomer Caspar Peucer advocated divination from meteorological and astronomical signs – an idea that is also present in the thought of Johannes Kepler.[19]

Is there anything specifically Lutheran about these providential and astrological views? Gábor Almási has suggested that the idea that astronomy leads to knowledge of divine providence is a commonplace in prefaces to and commentaries on Johannes Sacrobosco's textbook *On the Sphere* – even those from the Catholic world.[20] Some of these Catholic sources also claim that astronomical signs could be used for prognostic purposes. However, it may be objected that the three most influential Catholic thinkers mentioned by Almási – Jacques Lefèvre d'Étaples, Alessandro Piccolomini, and Clavius – take an unambiguously critical stance toward astrological prognostications. Lefèvre d'Étaples emphasizes the speculative nature and the Gentile origins of these practices.[21] Clavius refers to a series of authoritative refutations of astrology, most prominently the one developed by Giovanni Pico della Mirandola.[22] Piccolomini argues that the inconstancy and perpetual flux of events on the elemental level preclude any regularity of the efficient causation of heavenly bodies on terrestrial objects.[23]

Moreover, as far as I can determine, any reference to the concept of *special* providence is absent from the writings of Lefèvre d'Étaples, Piccolomini, and Clavius alike. To be sure, a reference to providence can be found in Lefèvre d'Étaples, but what he has in mind is the immutable regularity and uniformity of the motions of heavenly bodies that cause changes on earth:

[16] Methuen (1999, 106). On Melanchthon's natural philosophy, see Kusukawa (1995); Bellucci (1998).
[17] Methuen (1998, 125–128). [18] Vermij (2010).
[19] Field (1984); Martens (2000); Lanzinner (2003); Miller (2008); Boner (2013); Barker and Goldstein (2016).
[20] Almási (2014). [21] Lefèvre d'Étaples (1517, 1^{v}). [22] Clavius (1585, 5–6).
[23] Piccolomini (1568, 148–149).

> This [celestial] motion is uniform in itself; and in lower things takes every form through its efficacy. In the same way, God is unique and simple in himself; while in creatures multiple and multiform, according to the different degrees of things dependent upon him.[24]

Similarly, Piccolomini adduces theological considerations when he discusses the shape of the outermost celestial sphere. As he argues, it must be spherical because only a shape that does not have any beginning or end could express the perfection of its creator.[25] However, Piccolomini does not consider providence in connection with any singular natural events. Nor does one find a reference to special providence in Clavius's discussion of the nova of 1572.[26]

The criticism of astrological prognostication and the absence of reference to special providence thus constitute two significant differences between leading Catholic astronomers and the Lutheran theologians and astronomers. Thus, even if the idea that the regular motion of heavenly bodies is an expression of providence was common to both traditions, and even if astrological views are not entirely absent from the Catholic tradition, cosmology may be an area where the confessionalization thesis has strong plausibility.

Confessionalization and the Physics of the Eucharist

Another promising connection between specifically confessional theological doctrines and natural philosophy can be found in the widely diverging doctrines of the Eucharist. However, the connection is less clear-cut for Lutherans than it is for Catholics and Calvinists.

The Catholic doctrine of transubstantiation, according to which the substance of bread turns into the substance of the body of Christ, while the sensible accidents remain the same, evidently raises a question concerning how accidents can possess the required degree of independence from substances.[27] A good example of a discussion of this question is found in the physics of the Jesuit Honoré Fabri. Fabri claims that corporeal qualities can miraculously become accidents that can exist outside a subject, and in the Eucharist are corporeal qualities.[28] He conjectures that the body of Christ is penetrated by these accidents, which are themselves impenetrable and

[24] Lefèvre d'Étaples (1517, 13[r]). Unless otherwise noted, translations are my own.
[25] Piccolomini (1568, 13). [26] Clavius (1585, 192–194). [27] Hellyer (2005, 90–113).
[28] Fabri (1669, 8, 18).

extended.[29] Of course, one may wonder whether penetration is not a reciprocal relation: if accidents penetrate a substance, then the substance penetrates the accidents; also, if there are several accidents that penetrate the same substance, these accidents also seem to penetrate each other. Fabri seems to have been aware of the second problem since he suggests that the accidents are only impenetrable by accidents that are not relevant for what is going on in the Eucharist.[30] But he does not say what distinguishes the irrelevant accidents from other accidents. In any case, from these considerations he draws the conclusion that the body of Christ in the Eucharist has internal quantity, but not impenetrability.[31] A possible solution for the difficulties arising from the hypothesis of the independence of accidents from substances can be found in René Descartes's defense of transubstantiation. According to Descartes, the accidents that remain seemingly the same are nothing but secondary qualities, such that the new substance must only have primary qualities capable of producing the same sensory impression as the primary qualities of the previous substance.[32]

A strong connection between theological doctrine and natural philosophy can also be found in the case of the Calvinist doctrine of the Eucharist. According to John Calvin, what takes place in the Eucharist is a "showing" (*exhibitio*) of the body of Christ "because its truth is inseparable from the sign."[33] But in Calvin's view, this does not involve presence of the body of Christ in an invisible form because this is contrary to the nature of bodies. As he argues, if the body of Christ were ubiquitous by its nature, then its birth and existence in space and time would be due to some dispensation from its natural state. In this case, the local and temporal existence of the body of Christ "would be a *phantasma*."[34] For Calvinist natural philosophers, it was therefore essential to argue for locality as an essential property of bodies. Perhaps the most sophisticated version of this argument can be found in Clemens Timpler.[35] Timpler distinguishes the Aristotelian conception of place as defined by the surfaces of surrounding bodies from quantity as an essential property of created substances. He maintains that quantity as an essential property can be ascribed both to corporeal and incorporeal substances, independently of whether they are surrounded by anything.[36]

The connection between the theology of the Eucharist and natural philosophy is less clear among Lutherans. Lutherans used the outer-sphere problem (i.e., what is the place of the heavens?) to defend the view that

[29] Fabri (1669, 9). [30] Fabri (1669, 19). [31] Fabri (1669, 15). [32] Alexandrescu (2007).
[33] Calvin (1553, 182^{r}). [34] Calvin (1553, 182^{v}). [35] Leijenhorst (2001, 530–532).
[36] Timpler (1607, 120–126).

being in a place is not essential for bodies,[37] but this argumentative move contributes little to analyzing how more than one body can be at the same place at the same time. Something more specific is needed, and Luther suggests the body of Christ is present in the bread "definitively."[38] As Luther explicates his argument, according to the Scholastic theory of being in a place "definitively," a being can occupy, without undergoing any change, a smaller or larger body, such as when demons are thought to be capable of residing in a nutshell as well as in a whole house. Luther transfers this theory from spiritual to corporeal entities and conjectures that the body of Christ can be present behind the closed door of the grave and in the eucharistic bread. He understands this way of being in a place as an alternative to the concept of "circumscriptive" presence (presence in a place that is defined by the boundaries of surrounding bodies), as well as an alternative to the concept of "repletive" presence (the presence as a whole in every part of a creature, which Luther ascribes to God).[39]

As Giovanni Gellara has brought to light, the conception of definitive presence of Christ's body was indeed adopted by some Lutheran philosophers, such as Christoph Scheibler.[40] However, other theologians with Lutheran leanings found Luther's theory of definitive presence unsatisfactory. For instance, Philipp Melanchthon held that after the resurrection, the body of Christ has a single location in the heavens, and it is present insofar as it increases the perfection of the Church and vivifies the minds of believers.[41] Johannes Brenz, the reformer of Württemberg, takes an intermediary position. As he argues, due to the union of the powers of Christ and the person of the Father, the humanity of Christ is wherever God is. Since Brenz accepts the view that God is present in every creature, he also maintains that the body of Christ is present in all creatures, even though in a non-sensible way. What is special about the Eucharist, in Brenz's view, is that Christ has given only to bread and wine the function of signifying his sacrifice. In this sense, only bread and wine can show (*exihibere*) the body of Christ, although the body of Christ is present in all creatures.[42]

These doctrinal differences between Luther and Lutherans are highly relevant for the treatment of the Eucharist in one of the most sophisticated Lutheran natural philosophers, Tübingen-based Jacob Schegk.[43] Schegk offers an answer to the question of how God can be omnipresent without

[37] Leijenhorst and Lüthy (2002). [38] Luther (1534, sig. $k1^{r}$).
[39] Luther (1534, sig. $k1^{v}$–$k2^{r}$).
[40] Gellera (2013, 1104–1105). [41] Melanchthon (1559, sig. $H3^{r}$–$H4^{v}$).
[42] Brenz (1590, 510–512). [43] Frank (2003, 93–100); Haga (2012, 145–154).

being multiplied and limited according to particular places and particular objects by adopting the Platonic view that all creatures are prefigured in the divine ideas and that concrete beings are images of what exists in the divine mind. This is why he accepts the view that God exists in every point of the world as a whole, but he qualifies this claim by adopting the Scholastic theory of eminent containment according to which the effect is contained in the eminent cause in a more perfect way. Consequently, he holds that divine operations are present in creatures in a way that is not distinct from the presence of divine essence in creatures.[44] Moreover, in order to explicate the sense in which God can be present as a whole to the whole world and as a whole to each of the parts of the world, Schegk applies the relation between different sensible species to explicate the relation between different divine potencies: As different sensible species can be present in the same medium, different divine potencies can be present in the same place.[45] But this mode of divine omnipresence is compatible with the view that the body of Christ, "insofar as it is considered absolutely and with respect to its nature . . . is absent, and furthermore can exist only in a single place through natural actuality."[46] Consequently, the body of Christ cannot be co-extensive with other bodies. Schegk concludes that, in the Eucharist, there is no natural or local presence of Christ; rather, the Eucharist should be understood as a visible sign of invisible grace, particularly of the efficient role of Christ in salvation.[47] Schegk thus uses various elements of natural philosophy to explain the concept of divine ubiquity; but he rejects Luther's concept of the "definitive" presence of the body of Christ in the Eucharist. Schegk's work illustrates how the divergences between Luther and Lutheran theologians opened up a space in which Lutheran natural philosophers were able to explore a variety of positions concerning the physics of the Eucharist and likewise loosened the connection between confessional commitments and natural philosophy.

Confessionalization and Natural History

The two preceding sections have presented two areas where the confessionalization thesis has considerable plausibility, although it may require some qualifications concerning overlaps between Catholic and Protestant texts and concerning the internal divergences between Protestant theologians. By

[44] Schegk (1566, 44–48). [45] Schegk (1566, 60–61). [46] Schegk (1566, 50).
[47] Schegk (1566, 50).

contrast, in this and the following section, I present two fields that offer some counter-evidence. In the present section, I challenge the view that the disappearance of moral meanings from early modern natural history could be explained by some features of the hermeneutics of the reformers. In the following section, I explore how natural philosophy was used to counter the dynamics of confessionalization from an ecumenical point of view.

Peter Harrison provides extensive evidence for the disappearance of symbolic meanings from early works on natural history. The majority of his sources derive from the English tradition, but he also invokes the botanical work of the Zurich-based naturalist Conrad Gesner.[48] Harrison explains the disappearance of symbolic meanings of plants and animals through what he calls "the collapse of allegorical interpretation of texts." As he puts it, the "literalist mentality of the reformers . . . gave a determinate meaning to the text of the scripture, and at the same time precluded the possibility of assigning meanings to natural objects."[49] However, Barbara Johnston has pointed out that moral meanings are present in Gesner's zoological writings, as well as in the *Theatre of Insects*, a work published in London in 1634.[50] The latter work is built around unfinished notes by Gesner; one of his collaborators, the English preacher and physician Thomas Penny, added further material, and Penny's friend Thomas Moffett, a physician to English aristocracy, completed the text. Still, Johnston sees this, as well as Gesner's work on the history of animals, as intermediary steps in a development that led to a view of nature devoid of moral meaning. As it turns out, contrary to what Harrison claims, the moral interpretations of animals found in Gesner and Moffett are in *conformity* with the hermeneutic practices of the reformers. This is why the disappearance of symbolic interpretations of animals cannot be explained by how the reformers interpreted biblical texts.

There is a substantial body of work in the history of theology that documents the intricacies of the hermeneutic practices of the reformers, even though this body of research seems to have been largely neglected by historians of science. In particular, it has been made clear that Luther was aware of the fact that the biblical texts are full of metaphors, allegories, and other rhetorical figures.[51] Similarly, Johannes Oecolampad, the reformer of Basel, holds that biblical allegories combine a historical-literal sense with a prophetic sense that refers to the coming of Christ, as well as a moral sense that guides toward the virtues of charity and mercy, both in the biblical past

[48] Harrison (1998, 78–79). [49] Harrison (1998, 4). [50] Johnston (2011).
[51] Ringleben (1997).

and in the present.[52] Likewise, Huldrych Zwingli, the reformer of Zurich, thought that biblical metaphors and allegories can lead, by exploiting similarities between different topics, to insight into moral and prophetic truths.[53] What the reformers certainly were opposed to was the *arbitrary* interpretation of anything in the bible as a metaphor or an allegory; but where there is textual evidence that something was intended as a metaphor or an allegory, then they agreed that a textually and contextually informed decoding of the metaphorical or allegorical sense was called for.

Such a hermeneutic attitude is particularly relevant for the interpretation of the prophets, whose texts can hardly be reduced to a literal-historical sense. There, one finds a wealth of natural particulars – plants, animals, body parts, aspects of agricultural life – that are used in symbolic senses, and while the reformers disagreed about many of the details, they agreed that these passages require metaphorical and allegorical readings. To give just one illustration: the interpretation of a passage from Isaiah 1:3, where the disobedience of the Jewish people is contrasted with the observation that the ox and the ass recognize the one who gives them food and shelter. Luther is clear that the ox and the ass here are used as moral exemplars of gratitude[54] – which is only one instance of Luther's use of animals as moral exemplars.[55] Oecolampad compares the case of the gratitude of the ox and the ass with other animal virtues, such as the supposed mildness (*mansuetudo*) of lions toward their benefactors.[56] Calvin, too, assigns moral meanings to animals – in fact, often negative meanings.[57] Regarding the passage at hand, Calvin observes that brutes can learn and recognize those who feed them, which is what we can learn "in the school not of humans but of beasts." In particular, he holds that one can learn one's duty (*officium*) from them, since "often beasts follow the order of nature better and carry more humanity [*humanitas*] with them than humans themselves."[58] As he explains, animals do not feed on their own kind, and hence must be able to recognize their similarity to others; brutes employ care in raising their offspring, while human parents often forget nature and humanity and abandon their children. Also, brutes take as much food and drink as is sufficient for upholding life and forces, while humans consume superfluous goods. And brutes generally, in contrast to humans, do not transgress the laws of nature. Hence, Calvin takes the allegory of the ox and the ass to relate to the "order of nature."[59]

[52] Oecolampad (1525, 5^{v}). [53] Zwingli (1959, 148–149). [54] Luther (1860, 18).
[55] Clough (2009, 56). [56] Oecolampad (1525, 7^{v}–8^{r}). [57] Huff (1999).
[58] Calvin (1551, 9).
[59] Calvin (1551, 10).

If one compares these interpretations with the treatment of the moral signification of animals in Gesner, it becomes clear that Gesner's view is fully consistent with the hermeneutics of the reformers. Gesner cites approvingly from Theodore of Gaza's preface to the translation of Aristotle's *Historia animalium*: The habits of animals provide "exemplars of all duties [*officia*] and offer an image of virtues with the highest authority of nature."[60] Not only do brutes abstain from killing members of the same species, they also use food and sleep according to what their well-being demands. Ants and bees provide models of industriousness, while bees are models of justice because they collect only what they need without hurting any plant. This is also why animals can be used symbolically:

> To this can be added the usefulness that arises from these animals for the art of speaking. For the comparison and assimilations that the Greeks call parables, which excellently ornament speech and keep the attention of the audience, can be developed with variety, copiousness and aptitude from thence . . . .[61]

Altogether, the general view is that animals can be used symbolically *because* they exemplify natural goodness; and this is exactly what corresponds to the interpretation of the ox and the ass that one finds in the Isaiah commentaries of Luther, Oecolampad, and Calvin. In light of these textual observations, one could conclude that the hermeneutics of the reformers does explain something about the development of early modern natural history; however, what it explains is the *persistence* of the assignment of moral meanings to animals rather than the *disappearance* of such meanings from natural history.

Confessionalization and Ecumenical Natural Philosophy

Ecumenism was an early modern movement that explicitly sought to counter confessionalization, and its use of natural philosophy, most prominently in the work of the English Catholic natural philosopher Sir Kenelm Digby, is another obstacle to all-too-general confessionalization claims. This insight is easily obscured by the ambiguity of the concept of ecumenism. In one sense, ecumenism can be understood as an ecclesiological concept centered on the idea of the reunion of churches. As Stefania Tutino has pointed out, though, ecclesiology did not play a significant role in Digby's politics.[62] In another

[60] Gesner (1551, sig. c4^{r}). [61] Gesner (1551, sig. c4^{r}). [62] Tutino (2016, §3.2).

sense, ecumenism can be understood as centered on the concept of a "common theology." According to John Henry, Digby's *Two Treatises* contribute to an ecumenical program by providing an answer to the question of whether the human soul is naturally immortal – a question that divides Catholic dogma from the theology of the Church of England. Catholic dogma holds that human beings – composed of body and soul – are naturally immortal, while theologians of the Church of England tend to ascribe the immortality of both human bodies and human souls to a supernatural Divine intervention.[63]

This "common theology" approach may be investigated further in two directions. First, it may be interesting to note that a similar ecumenical approach to the use of natural philosophy for the purposes of defending the doctrine of the immortality of humans can be found, with explicit reference to Digby, in the early writings of the Lutheran Gottfried Wilhelm Leibniz.[64] Second, it may be instructive to ask why Digby and Leibniz took such a strategy to be convincing for members of different confessions. As I would like to suggest, Digby and the early Leibniz share the view that common notions could be used as criteria for the evaluation of natural philosophical hypotheses that are acceptable to members of different confessions. For instance, Digby suggests that we should prefer ordinary concepts: "we should acquiesce and be content with that naturall and plaine notion, which springeth immediately and primarily from the thing it selfe: which when we do not, the more we seeme to excel in subtility, the further we goe from reality and truth."[65] In his view, "[i]t is the indisciplined multitude that must furnish learned men with naturall apprehensions and notions to exercise theire wittes about."[66] Because they confer content on theoretical concepts, common notions also function as criteria for adequate theory formation – as Digby points out, they function as the "norm of discourse" (*norma loquendi*).[67]

Digby applies this strategy from the very beginning of the *Two Treatises*, when he points out that when thinking about body, "the first thing which occurreth to our sense in the perusal of it, is its *Quantity*, bulke, or magnitude: and this seemeth by all mankind, to be conceived ... inseparable from a body."[68] In other words, categorial concepts such as "quantity" do not belong to the technical vocabulary of specialists, but are rather part of ordinary language. Likewise, Digby holds that the common notion of quantity is nothing but the extension of a thing, "expressed by a determinate number

[63] Henry (1982, 223–227).
[64] On Leibniz's relation to Lutheranism, see Goldenbaum (1998); Backus (2016).
[65] Digby (1644, 4–5). [66] Digby (1644, 8). [67] Digby (1644, 8). [68] Digby (1644, 1).

of lesser extensions of the same nature," such that "the whole by comprehending those partes, is a meere capacity to be divided into them" and concludes that quantity is nothing but divisibility.[69] Moreover, Digby argues that the distinction between substance and quantity is also present in our "familiar discourse," when we, e.g., say that Socrates was bigger as a man than as a boy, or that boiling milk runs over the pot it is in.[70] Finally, Digby argues that quantity may be changed while substance remains unchanged. The cases of boiling milk or boiling water provide him with striking examples of a change of quantity in an unchanged substance.[71]

Similarly, central aspects of Leibniz's early views on the nature of matter are founded on an analysis of our everyday conception of material objects. Leibniz analyzes matter in terms of epistemically primitive concepts by searching for criteria by which we distinguish between bodies and non-bodies. According to his view, this criterion is resistance (*antitypia*) together with extension. As he argues, "whatever humans only sense to be extended or what they only see . . . they do not immediately call a body, for they think that sometimes it is only a mere image and *phantasma*. But what they not only see but touch, that is, in what they find *antitypia*, this is what they call body, while what lacks *antitypia*, they deny that it is a body. In two things both experts and laymen locate the nature of body, in extension and *antitypia* taken together."[72] Leibniz's approach to the nature of material objects includes an analysis of our immediately accessible thought about body and space. As he argues, space and body are distinct because

> we perceive that we think of space as the same when bodies change, and what we perceive ourselves to be thinking or not thinking we perceive truly. The perception of thought is immediate to the thought itself in the same subject, and so there is no cause of error. Therefore, it is true that we think of space remaining the same when bodies change and that we can think of space without a body which is in it. Now two things are diverse if one can be thought of without the other.[73]

As in Digby, one finds here an argumentative move from commonly shared ideas to the nature of space and body.

The crucial point of the analysis of matter – both in Digby and in the early Leibniz – is to argue that matter, due to its inherent passivity, cannot provide an adequate explanation for the active aspects of human thought. Thought, therefore, must be the property of an immaterial and hence immortal human

[69] Digby (1644, 9). [70] Digby (1644, 25). [71] Digby (1644, 25). See Blank (2007).
[72] Leibniz (1923–, 6-2:443). [73] Leibniz (1923–, 6-2:304–305); Leibniz (1969, 143).

soul. In fact, Digby describes the question of the immortality of the soul as the "main and great question" of his work.[74] At the same time, he understands an account of the actions of inanimate and living bodies as the "main hinge, upon which hangeth and moveth" this question.[75] In particular, he characterizes the goal of the first part of the *Two Treatises* as an account of the actions that bodies are capable of, thereby also indicating what actions bodies are not capable of. The task of the second part of the *Two Treatises* is to show that the actions that are specific to rational souls cannot be reduced to the principles governing the bodily world.[76] Digby claims that what he says about human souls expresses the "common notion of spiritual things."[77] To give just one example for such an argument based on common notions, Digby observes that the mind can be characterized as "a comparing power; for all his particular knowledges, are nothing else but respects or comparisons between particular thinges."[78] Since matter, as characterized in the first part of the *Two Treatises*, does not have the power of comparison, minds must be immaterial.

The early Leibniz thought that Digby's arguments could be integrated into his own project of promoting ecumenism through philosophical arguments for the immateriality and immortality of the human soul. In the *Outline of the Catholic Demonstrations* (1668–1669[?]), Leibniz mentions, in his plan for the sixth chapter, the proof(s) developed in Digby's *Two Treatises*.[79] Leibniz emphasizes the importance of directing the attention to something that is already known, even though in an unreflective way. Accordingly, he develops a line of argument for the difference between the structure of *conatus* in the mind and of *conatus* in bodies based on an analysis of the structure of thought. As Leibniz observes, "thought is nothing but the sense of comparison," and he makes clear that thought therefore involves two active aspects: (1) "*To think* is being the reason of change, or to change itself"; and (2) "*To think* is indefinable, in the same way as *to sense*, or rather *to act* are indefinable. And nevertheless, once one undergoes them, they are reflected in themselves."[80] The first active aspect of thought thus is due to the fact that every thought gives rise to further thoughts; the second active aspect is due to the fact that every thought is accompanied by reflexive awareness. Moreover, Leibniz regards the capacity of retaining and comparing its own states as a further active capacity that defines the nature of the mind.[81] Contrasting the description of the structure of thought with the common notion of matter indicates that the mind must be an immaterial and indestructible entity.

[74] Digby (1644, 342). [75] Digby (1644, 342). [76] See Digby (1644, 341–342).
[77] Digby (1644, 444). [78] Digby (1644, 360). [79] Leibniz (1923–, 6-1:494–495).
[80] Leibniz (1923–, 6-2:282–283). [81] Leibniz (1923–, 6-2:285); Blank (2005, §3.3).

The usage of common notions explains why Digby and Leibniz converge on a similar strategy of using natural philosophy for the purpose of identifying areas of belief that can be shared by members of different confessions. Natural philosophy thus provides rational grounds for a common theology and thereby fulfils the ecumenical function of countering the dynamics of confessionalization.

Conclusion

This chapter has offered a few observations that give an impression of how heterogeneous the relevant source materials concerning the relation between confessionalization and early natural philosophy are – too heterogeneous, probably, to invite any very general conclusions. What these observations indicate is that the relation between confessionalization and natural philosophy is different in different thematic areas. Natural philosophy indeed may be used to defend theological claims central to the program of confessionalization, such as the philosophical analysis of transubstantiation (or its impossibility) or the philosophical analysis of geocentric astronomy. But then it also may turn out that some theological claims prominent in one confession are by no means absent from works in different religious traditions, as seems to be the case with a providential reading of heavenly phenomena and astrological prognostications. And it may turn out that theological disagreements between theologians belonging to the same confession, as in the case of the disagreements between Lutherans concerning the nature of the Eucharist, opened up the theoretical space for philosophical conceptions that did not coincide with the position taken by Luther, as can be seen in the writings of Schegk. What is more, while the disappearance of symbolic meanings from natural history is certainly a characteristic feature of the emerging scientific approach to plant and animal physiology, explaining this development through Protestant hermeneutics may be inconclusive for the simple reason that Protestant hermeneutics did not eliminate the assignment of symbolic meanings to natural particulars. Finally, natural philosophy was also used as a tool for an ecumenical program that resisted confessionalization, as can be seen in the writings of Digby and the early Leibniz.

Can something be learned from this heterogeneity concerning the disciplinary identity of early modern natural philosophy? I do not want to contest Grant's argument that it does not make much sense to assume that early modern natural philosophers always were thinking about the relation between God and the natural world, when such a conjecture does not

make any difference to the interpretation of a particular text. Nevertheless, one should not dismiss the programmatic statements to the effect that the entire aim of doing natural philosophy is to gain a better understanding of God – even if such statements might indeed be nothing more than a response to an intellectual climate that, especially in Protestant regions, was by no means favorable to philosophical reflection. In spite of the diverging role of specifically confessional influences, all of the philosophical issues discussed here in some way or other do concern the relation between God and nature. Obviously, this is the case with the question of divine providence as an object of astronomy; it is the case with the question of divine presence in the Eucharist; it is the case with the question of animal virtues as an expression of a divinely created natural order; and it is the case in the question of the natural immortality of human souls. In this sense, there is in fact substantial textual evidence that even details of natural philosophy were driven by theological concerns. Still, as I have argued, not all aspects of early modern natural philosophy were motivated by theological concerns (as in the case of the disappearance of symbolic meanings from many works on natural history), and some theological concerns that influenced natural philosophy should not be described in terms of the specific mechanisms of confessionalization (as in the case of the ecumenical strategies found in Digby and Leibniz). Even the limited number of case studies considered here thus points to the conclusion that there may not have been anything that could be described as *the* confessionalization of early modern natural philosophy.

7

The Rise of a Public Science? Women and Natural Philosophy in the Early Modern Period

KAREN DETLEFSEN

Introduction

The late Renaissance and early modern revolution in thinking about the natural world occurred alongside an important sociological phenomenon. There was a sharp rise in intellectual activities, including the study of nature, outside private institutions that were largely under the control of the church and formally closed to most members of the public. That is, the early modern period saw the rise of a public science.[1] By the early nineteenth century, science as we now know it had come into being and superseded much of the old natural philosophy.[2] Yet this new discipline was marked in part by its association with new professional institutions that again excluded the public.

Closely related to this developmental story is one told by some feminist historians of science and philosophy, including Judith Zinsser and Eileen O'Neill. Zinsser links the increasing access to the study of nature to the cascade of women practicing science (in the loose sense of that word) in the early modern period. Before the late sixteenth century, according to Zinsser, scientific knowledge was learned, passed on, and practiced in academic institutions. During the revolutionary period, science was more often practiced by independent autodidacts who eschewed the intellectual confines of institutions, and this freedom resulted in both rapid intellectual development

[1] While I will use the more historically accurate term 'natural philosophy' regularly, I will also use the anachronistic and less accurate term 'science' for expressional ease when referring to practices involving the investigation of natural phenomena that were precursors to current-day science. I will do so especially when using common phrases, such as 'a public science'. Below, in light of discussion in the fourth section ("Kinds of 'Science'"), terminological issues become yet more complex.

[2] E.g., Ross (1962); Cunningham (1988a).

and the ability of those traditionally excluded from institutions (women, for example) to participate more fully as scientists. As Zinsser writes, "The old authorities, such as the faculties of the universities, found their knowledge and their methods of reasoning questioned . . . . In these circumstances women of the privileged classes . . . had an opportunity to engage in the intellectual discourses of their day."[3] Zinsser does not mean "to contradict historians' depiction of the masculine traditions of Europe's intellectual history, and the subsequent gendering of learning; rather . . . for a brief period those traditions and that gendering could be made less consequential."[4] The re-institutionalization of natural knowledge in the late eighteenth to early nineteenth centuries contributed to the rolling back of the gains enjoyed by women. As Zinsser argues, "New institutions, intellectual authorities, such as the Royal Societies and learned journals, had emerged in many European capitals, and women, who at first had actively participated in scientific activities and speculation, found themselves marginalized."[5] O'Neill, too, has argued forcefully for the blossoming of early modern women's philosophy, at least in part because of "the blossoming of philosophical activity outside of the schools since the Renaissance,"[6] and given that philosophy includes *natural* philosophy, this argument can be extended to support Zinsser's idea.

This chapter focuses on the intersecting ideas of science becoming more public and of women becoming more involved in science throughout the seventeenth and early eighteenth centuries. Specifically, this chapter tracks the suggestion that when science belonged to exclusive, private institutions, it was dominated by men, and when science became more public, women became practitioners in greater numbers. There is surely much truth to this characterization, but the aim of this chapter is to complicate this narrative. What is meant by 'public', and its supposed opposite, 'private'? What role is played by institutions in this story, and what is meant by an institution? Indeed, what is meant by 'natural philosophy'? By interrogating some aspects of questions such as these, we can gain a richer understanding of the roles that women played in the sphere of natural philosophy during the so-called Scientific Revolution.

I will proceed as follows. In the next section, I discuss two women, Johanna Stephens and Émilie Du Châtelet, working in medicine and in natural philosophy in the mid-eighteenth century. This was the twilight of the Scientific Revolution, as its Newtonian culmination gained a hold across Europe. It was also when study of the natural world began to move back

[3] Zinsser (2005a, 4). [4] Zinsser (2005b, 52). [5] Zinsser (2005a, 4–5).
[6] O'Neill (1998, 19).

into institutions, most notably scientific institutions such as the Royal Society and the Académie Royale des Sciences. So these two figures were active at a time when, if the above narrative is correct, women started to become more marginalized precisely because of the waning of a public science. I examine Stephens's contributions to medicine, in the form of her cure for bladder stones, and her subsequent fate. Regarding Du Châtelet, I examine two features of her work. The first is her discussion of women's engagement in intellectual life, especially against a backdrop of limited educational opportunities. The second feature is the history of her engagement in the *vis viva* controversy, focusing on her publications on the topic and her interactions with the secretary of the Académie, Jean-Jacques Dortous de Mairan. In the subsequent sections, I turn to the central issue of this chapter – the supposed rise of a public science and women's role in that transformation – to underscore a number of ways in which the issue is considerably more complicated than the simple characterization above, as exemplified by Stephens and Du Châtelet.

Two Women Scientists

Stephens's Cure for Bladder Stones

Little is known about the life of Stephens. What is known is that in 1740 she was paid the then-enormous sum of £5,000 by the governmental Board of Longitude as a prize acknowledging the value of her cure for bladder stones. This painful condition was usually treated with a relatively dangerous surgery, but Stephens's cure was taken orally. In 1738, David Hartley – himself a sufferer of stones – had published *Ten Cases of Persons Who Have Taken Mrs. Stephens's Medicines for the Stones*, which lauded Stephens and detailed his own and others' benefits from the medicine. Stephens then offered to reveal her cure for £5,000, an offer not enthusiastically taken up by the public. This prompted accumulation of 155 further testimonies from those who had been cured. At that point, it was decided that the £5,000 would be paid if the College of Physicians in London could verify the effectiveness of the medicine. By May 1739, £1,356 had been collected by 189 members of the general public, and Stephens appealed to Parliament, who agreed to contribute the balance if the cure was proven effective.

In the June 16, 1739, edition of *The London Gazette*, Stephens published the full recipe for her medicine, including the methods of preparation and administration. Thereupon, Hartley and Stephen Hales tested the cure at the Royal Society, while S. F. Morand and

C. J. Geoffroy investigated it at the Académie Royale des Sciences. The testing was experimental and interventionist. In Hartley's experiments, stones were immersed in various liquids, Stephens's among them, and the relative weight lost by the stones was recorded. Geoffroy analyzed the medicine chemically. Morand's approach was close to Hartley's, and he added hypotheses about the causal mechanisms of the cure. Hales aimed, through various experiments, to determine the relevant agent in Stephens's medicine and to create a cure that included only the necessary ingredients, publishing his findings together with those of Hartley, Morand, and Geoffroy in *An Account of Some Experiments and Observations on Mrs. Stephens's Medicine* (1741).

Taking up this case, Stephen Clucas writes that it is "an apparent success story," since Stephens was able to secure £5,000 for her work.[7] But, as Clucas emphasizes, the success is merely apparent, because as soon as the recipe was published, it essentially became the intellectual property of the men who undertook, in traditional scientific institutions closed to women, experiments to determine the source of its efficacy. As Clucas points out, Stephens's positive role in the history of her remedy was all but eliminated, even while she has too often been included in "the history of quackery."[8] Rather, men have enjoyed the positive historical treatment of her medicine. As a stark example, in his comments to the Royal Society in January 1742, John Rutty reports on Hartley's and Hales's experiments to their advantage and to Stephens's disadvantage:

> Thus these learned Gentlemen have, in some measure, rescued this Medicine from the Imputation of being merely Empirical; and indeed our whole Faculty must acknowledge ourselves in a particular manner indebted to the accurate Dr Hales for his ingenious Experiments.[9]

Says Clucas, "Stephens's medicine, then, is in itself 'merely Empirical', rendered 'accurate' and scientific by Hales's 'ingenious experiments'."[10] In brief, Hartley and Hales – and Morand and Geoffroy – remained in the scientific and public consciousness after Stephens was awarded £5,000. By contrast, "Joanna Stephens disappears from the story, and except when she and her medicine were attacked or defended by her critics, she is never heard of again."[11]

[7] Clucas (2005, 147). [8] Viseltear (1968, 200); Clucas (2005, 156). [9] Rutty (1742, ii).
[10] Clucas (2005, 151). [11] Viseltear (1968, 205–206).

Du Châtelet on Men, Women, and Education

Before addressing a specific case study drawn from the work in natural philosophy for which Du Châtelet is rightly best known, I turn briefly to a somewhat less well-known aspect of her work – her discussion of the importance of education for participation in intellectual matters, natural philosophy included. Du Châtelet had no institutional education early in her life, but she did have access to it later and indirectly through her well-educated tutors in mathematics and physics. Still, Du Châtelet laments her late coming to learning, sometimes implicitly, sometimes explicitly.

For example, Du Châtelet opens her masterwork on natural philosophy, *Institutions de physique*, with a direct concern about education. Indicating that the work is intended as a textbook for her son, she writes to him:

> I have always thought that the most sacred duty of men was to give their children an education that prevented them at a more advanced age from regretting their youth, the only time when one can truly gain instruction. You are, my dear son, in this happy age when the mind begins to think, and when the heart has passions not yet lively enough to disturb it . . . .
>
> You must early on accustom your mind to think, and to be self-sufficient. You will perceive at all times in your life what resources and what consolations one finds in study, and you will see that it can even furnish pleasure and delight . . . .
>
> Guard yourself, my son . . . against the inevitable obstinacy to which the spirit of [national] partisanship carries one; this frame of mind is dangerous on all occasions in life; but it is ridiculous in physics [and] the search for truth . . . .[12]

Here, Du Châtelet is explicit that education in one's youth is crucial for developing a mind that can own itself rather than too easily be swayed by the influence of others, even while, in this passage, she does not identify herself as one who has been deprived of such an education.

That insight into her own condition comes elsewhere. In her preface to her translation of Bernard Mandeville's *Fable of the Bees*, Du Châtelet addresses the issue of women's minds and education, including reflections on her own state:

> I confess that if I were king . . . I would allow women to share in all the rights of humanity, and most of all those of the mind. . . .
>
> I am convinced that many women are either ignorant of their talents, because of flaws in their education, or bury them out of prejudice and for

[12] Du Châtelet (2009, 116, 119–120).

> lack of a bold spirit. What I have experienced myself confirms me in this opinion. Chance led me to become acquainted with men of letters, I gained their friendship, and I saw with extreme surprise that they valued this amity. I began to believe that I was a thinking creature. . . . I only believed in earnest in my capacity to think at an age when there was still time to become reasonable, *but when it was too late to acquire talents*.
>
> I hold myself to be quite fortunate to have renounced in mid-course frivolous things that occupy most women all their lives, and I want to use what time remains to cultivate my soul . . . .[13]

One may dispute – as I will below – Du Châtelet's overly harsh self-assessment. But for now, the point is clear: Du Châtelet emphasizes the importance of education, most especially early in one's youth, for those engaged in intellectual life, including scientific work.

Du Châtelet and vis viva

Whatever Du Châtelet may have claimed, recent investigations of her work have recognized her prodigious talents. Here, I look at one small slice of her contributions to natural philosophy from a specific angle; namely, the publication history of her interventions in the *vis viva* controversy. My goal is not to detail the philosophical aspects of her contributions, but rather to show how she entered into the debate, and especially, how her interjections positioned her with respect to Jean-Jacques Dortous de Mairan, secretary of the Académie Royale des Sciences, from which Du Châtelet was excluded due to her gender.

The *vis viva* controversy lasted for several decades and spanned a wide range of intellectual and extra-intellectual issues. Here, it suffices to reduce it to a debate about the force acquired by a body in motion. Cartesians held that this was momentum, the product of mass and velocity (mv). Leibnizians asserted it was *vis viva*, mass times the velocity squared (mv^2).[14] By Du Châtelet's day, there were two towering figures in the debate in France: Johann Bernoulli, on the Leibnizian side, and Mairan, on the Cartesian.

In early publications addressing the *vis viva* question, Du Châtelet favors the Cartesian view. The first version of her *Dissertation on the Nature and Propagation of Fire* (1739) characterizes force as mv, specifically citing Mairan's defense of this conception in his 1728 entry in the *Mémoires*.[15] But even as this

[13] Du Châtelet (2009, 48–49, emphasis added).
[14] For examples of work that shows the complexity, including complexities in terminology, see Terrall (2004); Smith (2006a). See also Rey in this volume (ch. 24).
[15] Mairan (1728).

version went to press, Du Châtelet changed her mind. Though she could not alter the main text, she included a list of desired changes, and the abandonment of mv in favor of mv^2 was among the most significant. The 1744 version of the *Dissertation* was suitably updated.[16]

Du Châtelet challenged Mairan's view directly in her *Institutions* (1740). In the final chapter, she claims that "some refuse to accept *forces vive* while acknowledging the experiments that establish them,"[17] pointing to an incoherence in such positions, and thus leveling an especially harsh criticism. She then supports her claim by examining the ideas of specific individuals, starting with Mairan.[18] These sections represent a scientific take-down of the highest intellectual quality, laced with a fair dose of disdain. The ensuing back-and-forth was very public. Mairan defended his position and repudiated Du Châtelet's in a published letter, which prompted a reply from Du Châtelet, also as a self-standing publication.[19] Du Châtelet included this correspondence in both the 1742 revised edition of the *Institutions* and the 1744 revision of the *Dissertation*.

Du Châtelet thus participated in the *vis viva* controversy just as, according to the narrative under consideration in this chapter, science moved out of the public sphere and back into private institutions closed to women. Du Châtelet's confrontation with Mairan strategically pulled the debate out of the private and closed Académie into a sphere where she, as a woman, could engage. By making its way into widely available and accessible publications, the dispute, in Mary Terrell's words,

> had escaped the [scientific] bounds of the Academy and moved out into the Republic of Letters . . . . For Du Châtelet . . . vindicating Bernoulli by pointing out the weakness of Mairan's argument gave her a way to claim some scientific territory for herself. Her sophisticated treatment of Bernoulli's dynamics was something of a slap at the Academy . . . .[20]

Whether this "territory" was in fact *public*, and if so, in what way, will be treated below.

Women and Science: The Public, the Private, and Institutions

Bearing in mind the examples of Stephens and Du Châtelet, I turn to the intersecting ideas of this chapter. Recall, these are, first, the idea that the

[16] Du Châtelet (2009, 77). [17] Du Châtelet (2009, §573, p. 188).
[18] Du Châtelet (2009, §547–576, pp. 188–191). [19] Du Châtelet (1741).
[20] Terrall (2004, 201).

practice of science became more public throughout the seventeenth and early eighteenth centuries because that practice moved out of exclusive institutions, and, second, the idea that women became more involved in science because of this public turn. I use the above examples to interrogate aspects of this story to show how complex it really is. Again, my investigations here are limited, and the following is not a complete account. But it is enough to productively problematize the story of the rise of a public science and the increase of women in scientific investigations of the early modern period.

The Public and the Private: Institutions and Education

In the first place, let us accept the general characterization of the early modern period as one in which at least some scientific practice occurred outside exclusive, private institutions, thus allowing greater numbers of women to participate. This is the general idea behind Zinsser's and O'Neill's argument, and it tracks a common characterization of the period as one of radically independent individuals contributing to intellectual life. Yet even with this understanding intact, we can see how institutions nonetheless exercised power over the supposedly independent intellectual.

The persona of the independent practitioner is one crucial feature of the narrative of the rise of a public science. J. B. Shank, for example, referring to Descartes as a classic case of the independent intellectual, says he

> represented a new species . . . . [He] claimed the authority to speak about natural philosophical questions (including those, like mind/body dualism, that contained potent theological implications) without the sanction of traditional scholastic and ecclesiastical authority (the two were conjoined in the early modern French university).[21]

Shank also cites Descartes's influence when he identifies the seeds of a future "equality feminism" in the seventeenth century. This form of feminism, Shank argues, takes the essential equality of men and women as its starting point, and it receives one of its first sustained treatments by François Poullain de la Barre in his *De l'égalité des deux sexes*. "'The mind has no sex,' he famously declared," writes Shank, "and thus there was no reason that women could not equal men in all branches of learning so long as society gave them the education and support necessary to develop their talents."[22] Again, the paired ideas under investigation come to the fore: first, that the early modern period saw a shift in philosophical thinking and practice when it

[21] Shank (2005, 92). [22] Shank (2005, 97), citing Poullain (1673).

moved from (private) institutions to the much more inclusive public sphere; and second, that women benefited from this shift in the sense of being (at least in theory) considered the equals of men, given that they were among those excluded from the private institutions.

But Poullain's reference to women's education alerts us to complications with the conception of the radically independent intellectual free from association with intellectual communities within exclusive educational institutions. Descartes himself received a superior education in a Jesuit school, even if he then departed from the teachings of the institution.[23] The pervasive role of universities in the early modern period has been well-documented by John Gascoigne, who shows that a large number of figures central to the so-called Scientific Revolution were educated within, and otherwise closely associated with, universities, thus throwing the independent intellectual ideal into doubt.[24] In short, while there may have been plenty of extra-institutional education going on in the seventeenth and eighteenth centuries, formal academic settings still exercised enormous influence over who had intellectual standing and who did not. Precisely because very few women had the resources to obtain private education, and fewer still were permitted access to institutions of higher learning, the opportunities for women to be *effective* independent intellectuals were sharply circumscribed.

This conclusion is, of course, exactly the point of Du Châtelet's musing on women and education. While she received some of the best tutoring (especially in mathematics) money could buy – from Pierre Louis Maupertuis, Alexis Clairaut, and Johann König, for example – this occurred later in life, and only as a result of her own pursuit of such an education, which stemmed precisely from her exclusion from the private institutions her male counterparts enjoyed. Such deprivation was typical, even during the heyday of the Scientific Revolution a century earlier (when Poullain was writing), as the careers of Mary Astell, Gabrielle Suchon, Margaret Cavendish, and many more illustrate.

This is the first layer of complexity laid upon the narrative of the rise of a public science and the increase in women participating during the Scientific Revolution. While disciplines that are precursors to modern-day science may have been practiced outside of private educational institutions, and thus more in the public sphere, those educational institutions continued to

[23] I assume here that Descartes did depart in some ways from the ideas propounded in Scholastic institutions, though this is perhaps a contentious assumption. See, e.g., Dear (1995); Barker and Ariew (1999); Lefèvre (2001).

[24] Gascoigne (1990).

exercise enormous influence over the relative capacity of people – men and women – to partake fully in scientific intellectual life.

The Very Meanings of 'Public', 'Private', and 'Institution'

A second layer of complication arises when we consider the meanings of terms I have been using. What do we mean by 'private' and its supposed opposite, 'public'? When we discuss institutions, surely schools and scientific societies are obvious examples, with their names, edifices, enrollment criteria, and memberships. But are there other examples that might complicate the narrative of the rise of a *public* science free from enclosure in private *institutions*? My goal of showing the complexities surrounding the idea of the rise of a public science, and the implications for women, can be achieved by examining a few features of these nebulous questions.

I start with two ways of thinking about the private–public distinction. According to the first, 'private' denotes a practice or an institution that is not open to all, and is thus exclusive. Conversely, 'public' denotes a practice, or an institution, that is open to all, and is thus inclusive. According to the second way of thinking, 'private' refers to the intimate; for example, the family or home. Conversely, 'public' refers to the sphere outside home and family. We can weave gender through these two conceptions as follows. The first conception – the private as exclusive – has been treated in this essay as the sphere of men; private educational institutions and scientific societies were the province of men and not of women, and this accounts for the sidelining of women, at least until the rise of a public science. The second conception – the private as intimate – intersects with gender in that the home was often conceived of as being the proper place for women, while the public was seen to be properly under the control of men. The rest of this sub-section will interrogate the first conception, and some attendant ideas about institutions. The second conception will occupy the next sub-section.

Laying out this conceptual terrain helps expose further complexities to the narrative of the rise of a public science. One complexity is addressed by Shank, where he deals with the struggle in the 1680s between the academies and the salons – a "struggle that pitted the 'old guard' of the French academic establishment against the young turks of the salons."[25] "[I]t is clear," Shank writes, that this was "an *institutional* struggle over the control of knowledge."[26] Here, Shank rightly allows for a fairly broad understanding of the nature of institutions, which Zinsser also suggests when she includes,

[25] Shank (2005, 96). [26] Shank (2005, 97, my emphasis).

for example, "learned journals" among the institutional powers-that-be.[27] While one side in the struggle was associated with more traditional academies, the other was associated with more loosely organized salons and the enormously influential journal, the *Mercure gallant*.[28] Moreover, this "institutional" struggle was at least partly codified in gender terms:

> Ruling assumptions about proper discourse and sociability certainly distinguished salons from academies in the seventeenth century, but gender actually constituted the more powerful force shaping this divide. Academies as they developed in the seventeenth century were enclaves of all-male sociability . . . . Salons, by contrast, were sites of mixed-sex sociability where women enjoyed power in their roles as supervisors of taste, decorum, and cultural authority.[29]

Immediately, the idea of an institution becomes more nuanced. It may be that the academies ultimately prevailed, but they were not the only institutions exercising influence over the world of ideas in early modern Europe. As Shank indicates, salons (which continued to exercise influence into the eighteenth century) were more loosely organized and less formal institutions, which were governed by their own set of social norms, including who could participate, and under a relatively significant degree of female control. So, considering the narrative of the rise of a public science that allowed women more participation in the science, one might argue that there also existed exclusive – i.e., *private* – but nevertheless women-friendly institutions in which intellectual activities took place, including discussions of a scientific nature and the forms of knowledge they produced (often just as central as those produced in male-centered institutions), which lost out in the struggle for the control of knowledge production.

We might also consider that journals and other publishing venues were another sort of institution, which likewise complicates the story under consideration. Du Châtelet's publishing history in the *vis viva* controversy is relevant here. Precisely because of the private (as in exclusive) nature of the Académie, which precluded her from participating in that forum, her strategy was to move that debate into a different sphere – a different institution. This was not fully inclusive in that not everyone could gain access to publishing venues. And yet this sphere was not private in the way of the Académie. It did not, for example, exclude Du Châtelet simply because of her gender.[30] In light

[27] Zinsser (2005a, 4–5). See below. [28] Shank (2005, 95). [29] Shank (2005, 94).
[30] Wilda Anderson (private correspondence) suggests that publishing may have been less important during Du Châlelet's time, while circulating manuscripts among intellectual elites may have been more important. This would explain why Du Châtelet's *Optiques*

of these complicating cases, we can see that greater public access to scientific practice leading to greater participation by women is not the whole story.

The Home as Private: Women and Science

Let us now consider the other conception of 'private' offered above – the intimacy of home or family. Understanding 'private' in this sense also complicates the idea of the rise of a public science allowing greater participation by women, for the home seems one obvious place where women could more easily participate in scientific activities. Alix Cooper argues that "many of the activities crucial to the Scientific Revolution took place . . . within the seemingly humble and prosaic spaces of natural inquirers' own homes and households . . . Few historians of science have paid attention to these kinds of 'private' spaces."[31]

Cooper goes on to address the role of entire families, including females, and the division of scientific labor within families engaged in scientific projects. Among those discussed is the Winkelmann-Kirsch family, whose case is interesting because of the gendered aspects of their scientific work, especially how credit for the family's scientific work was assigned. Monika Mommertz has described how Gottfried Kirsch, the husband of Maria Margaretha Winkelmann-Kirsch, became the astronomer of the Berlin Academy in the early eighteenth century. After his death, he was succeeded by his son Christfried. Throughout the men's official association with the Academy, the women of the household contributed significantly – but unofficially and through work based in the household – to the Academy's work. Their contribution took two main forms. First, they were active participants in the men's home-based astronomical observations since "there was not enough space at the Academy to perform observations."[32] Second, they produced the Academy's astrological calendars, a lucrative activity – for the Academy – which continued even after Christfried's death severed the family's official association.[33] There was, then, a bifurcation of the activities of the Berlin Academy into those, on the one hand, officially undertaken and paid for, conducted largely on-site, and publicly recognized, and those, on the other hand, undertaken and paid for unofficially, carried out entirely within the household, and completely ignored publicly. Moreover, this bifurcation had a gendered aspect:

has been found in, for example, the Bernoulli archives even though it was not published during her own lifetime.

[31] Cooper (2006, 224). [32] Mommertz (2005, 166). [33] Mommertz (2005, 170).

> We are dealing with two interdependent work systems, which are characterized by a single economy of complex, flexible, and reciprocal-hierarchical interrelations. The relationships between these systems were not permanent and could be regulated by gender. The development of household production sites in connection with the institutional demands of the Academy shows that the scientific field of astronomy, the household, and gender were related to each other functionally.[34]

Considering the female family members' contributions, Mommertz notes "how the achievements of these women were fundamental to the continuance of the Berlin Academy in the eighteenth century, but also how the Academy consciously tried to render these achievements invisible."[35]

Returning to Stephens's case, one might ask where and how she developed her cure for bladder stones. Her page-length description of the medicine in *The Gazette* – its ingredients, method of preparation, and how it should be administered – does not disclose this information. Hartley says that Stephens had discovered the cure accidentally some twenty years before she gained fame for it, but not precisely *how*.[36] Rutty briefly mentions her London hospital,[37] an intriguing addition to our limited knowledge of her life, though Rutty does not address whether her hospital included forms of healing in addition to her bladder stone medicine. It is entirely possible that Stephens developed her cure in the *private* confines of her home; certainly, many women did practice science in this way. But she also ran, for a time at least, a hospital where people went to receive her medicine, and thus practiced outside of the home, as well.

Two lessons from the Winkelmann-Kirsch family drawn by Mommertz are informative here. The first is that the very real possibility that Stephens practiced science in the privacy of her home problematizes the general story line that the emergence of a public science enabled women as scientific practitioners during the Scientific Revolution. Certainly, freeing scientific investigations, at least somewhat, from private institutions that barred women enabled women to practice science, but there were other private venues, such as families and homes, that contributed to that enabling, as well. The second lesson harkens back to Clucas's point, detailed above, that despite Stephens's (and the Winkelmann-Kirsch women's, and surely many others') significant scientific contribution, it is not she who gained historically remembered credit. That went to the men who tested her cure within the private

[34] Mommertz (2005, 172–173). [35] Mommertz (2005, 161). [36] Hartley (1740, 37).
[37] Rutty (1742).

institutions from which she was barred. In other words, some private institutions, such as scientific societies, had far greater power to confer lasting reputations than did other private institutions, such as families and homes. The last two sections turn to some reasons why this imbalance obtained, thus tackling the question of why Stephens, and so many other women, were written out of the history of natural philosophy to which they rightfully belong.

Kinds of 'Science'

Many historians and philosophers in recent decades have challenged the narrative of the Scientific Revolution. One such objection maintains that the revolution was in natural philosophy, not science, since only the former was an extant discipline at the time. In this section, I expand on this challenge in order to provide some insights into the question of why women practicing in a discipline that was a precursor to science were so easily erased from our histories.

The guiding assumption behind the idea that the Scientific Revolution was a revolution in science – 'science' meaning then roughly what we mean by it today – is that a way of thinking is shared across historical times and geographical places. We can, as a result, understand ourselves and our place in scientific history by studying the past (especially the start of modern science in the seventeenth to early eighteenth centuries), and by seeing our position as the endpoint in the more or less linear unfolding of ideas. Similarly, we can understand past figures (especially those of the seventeenth century and later) in terms of our own current scientific practice because those at the dawn of modern science bequeathed us those practices.[38]

Critics believe this guiding assumption to be false and stress instead the importance of paying heed to actors' categories – that is, the way historical actors themselves named and described their activities.[39] This attention to the categories of the past has been central in the realization that *science* is a relatively new discipline, and that those before the nineteenth century did natural philosophy, or "chymistry," or mechanics, and so on. But it is not right to say that 'natural philosophy' was simply the name given to what we call 'science'.[40] These were distinct endeavors with different scopes,

[38] See, e.g., Cunningham (1988a); Cunningham and Williams (1993); Dobbs (2000); Osler (2000b); Westfall (2000).
[39] E.g., Dunn (1968); Skinner (1969); Wilson and Ashplant (1988).
[40] E.g., Cunningham and Williams (1993, 421); Hatfield (1996, 496).

responding to different questions and problems, and with different aims. As historians, then, we should avoid thinking about the history of science in the centuries before the nineteenth century as simply the history of natural philosophy. (Indeed, strictly speaking, the history of science begins in the nineteenth century.[41]) So, paying heed to actors' categories, one might point out that there were a number of disciplines that bear a family resemblance to later science, and that natural philosophy was just one such discipline. As the preceding section shows, the kind of "science" that women practiced in the privacy of their homes is yet another example of a discipline with a family resemblance to later science.

Moreover, women had a much greater role in the kind of scientific practice that took place in the home than they did in natural philosophy, at least in part because advanced education in theoretical matters was less important in the practical forms of science women like Stephens, for example, were involved with. Heeding actors' categories, then, suggests something like the following. In natural philosophy (as just one of many early modern disciplines bearing a family resemblance to current-day science), women were mostly marginalized, with a slight respite around the seventeenth century, and this marginalization was due to the exclusion of women from educational and scientific institutions. In other, more practical disciplines, women enjoyed far greater freedom to participate. But it was the theoretical natural philosophy – the province more often of men than of women – that was explicitly valued, and that consequently was carried forward by history. Clucas notes that Stephens was often dismissed because, while she may have possessed "knowledge and skill," she did not possess "scientific" knowledge because she did not understand theoretically how her medicine worked.[42] This was the gist of Rutty's dismissive evaluation of Stephens's work as "merely Empirical," quoted above.

The contrast with Du Châtelet is striking. Despite her lack of early education, she vigorously pursued an education in mathematical and technical matters throughout her adult life. As a result, she was exceedingly adept at understanding, practicing, and writing about natural philosophy, including the metaphysical, epistemological, and methodological underpinnings of empirical phenomena, which she in turn understood with exquisite technical prowess. She was, that is, thoroughly acquainted with the theoretical

[41] I do not intend to indicate that historians of science always do this, for some are well aware of the range of activities that went on in the early modern period which might legitimately count as pre-science; e.g., Cunningham and Williams (1993, 421).

[42] Clucas (2005, 147).

foundations of mathematically sophisticated physics. Du Châtelet enjoyed enormous respect, and her thought and work was well-known across Europe during her lifetime. Her *Institutions* was translated into German and Italian. Sixteen substantial articles in the *Encyclopédie* were based, almost verbatim, on her scientific writing. Her contributions to scientific theory and practice, and the recognition of her contributions, stand in stark contrast to Stephens's treatment, partly because of the distinction between different ways of doing science in the early modern period. But this distinction is orthogonal to the distinction between private and public science, and as such, it complicates in enriching ways the narrative that parallels the rise of a public science to the rise in women's involvement in science.

Modes of Communicating: Private, Public, or Not at All

A second complicating reason why some scientific work done by women was often devalued and dropped from our historical memory turns on the sometimes very different modes of communication exhibited by men and women in the early modern period. Lynne Hunter examines this phenomenon:

> men and women practiced science in the same places and with roughly the same equipment up until the middle of the seventeenth century. However, they practiced science for different reasons, leading them to communicate in different ways, and these different rhetorics have had a long-term impact on access to scientific power and to the legitimation of particular methodologies and of various kinds of scientific knowledge . . . . Because . . . women worked in communal locations [their family environment, their community, within the confines of the English country house, or among groups of practitioners], the notion of testing favored by modern scientists as replicable visual display was not relevant. And because their communication was primarily either oral or tacit, and by informal apprenticeship and observation, there was little need for the formal rhetorical strategies of written proof.[43]

Conversely, men did engage in testing and providing written proofs for their work, and rather than conceive of themselves as belonging to local communities, they thought of themselves as belonging to regional, national, and international communities of scientists.[44]

[43] Hunter (2005, 123, 131–132). [44] Hunter (2005, 126).

The divergent fates of Stephens and Du Châtelet can be partly explained by these different kinds of communication. It is surely the case that Stephens did not communicate widely, and in writing, about her cure, or at least not until she needed to in order to claim her £5,000 prize. That we know so little about how she developed the cure, where she developed the cure, and her thinking behind why it might work, is testament to this fact. That Hartley (alongside other men) prosecuted her case in widely read publications further underscores the role that communication forms can play in the uptake and attribution of ideas.

By contrast, Du Châtelet's modes of communication place her squarely in the tradition Hunter associates with men. Du Châtelet presented her ideas by, in Zinsser's characterization, "adopting the practices of her male contemporaries."[45] These included writing in a style more like other natural philosophers', discussing her work with men, acknowledging others' ideas and working methodically through her disagreements with them, and so on. As a result, she was often "acknowledged as the possessor of intellectual qualities usually associated with men."[46] Moreover, her savvy strategy for negotiating the power relations in the scientific world of mid-eighteenth-century France was clearly attuned to the power of certain modes of communication. By drawing Mairan out, so to speak, of the private and male-only confines of the Académie, and into the more public realm of publications to which a woman could contribute and to which a wide swath of Europe's intellectual elite could gain access, she cleverly harnessed the power of more traditionally male modes of communication, to the great benefit of her scientific reputation.[47] And so, the difference in Stephens's and Du Châtelet's receptions may be partly due to their different modes of communication. But again, this cuts across the public/private distinction at the root of the usual narrative.

Conclusion

For understanding the study of nature and women's roles therein, there is much of value in the idea that the early modern period saw the rise of a public science. I do not take this narrative to be a false characterization of an important phenomenon in the seventeenth to eighteenth centuries.

[45] Zinsser (2005b, 52). [46] Zinsser (2005b, 52).

[47] The story of her erasure from the historical record until recently is a separate one that does not impinge on her positive reputation during her lifetime. O'Neill (1998) tells this story, with respect to women philosophers in general.

At the same time, as is to be expected given the scope and nature of the changes to philosophical thought and to thought about the natural world, there are layers of complicating considerations. The fate of two of the many women practicing science in the period elucidates just a small handful of those complicating considerations.

PART II

⋆

DISCIPLINARY ACTIVITIES

8

The Art of Thinking

SORANA CORNEANU AND KOEN VERMEIR

"Logic in the modern phrase is the Art of Thinking, a help or instrument of reason." Thus wrote the Cambridge antiquarian Thomas Baker in his *Reflections upon Learning* (1699), an "Historical Account" of philosophy, conceived as an overview of the successes and failures of various ancients and moderns in each branch of learning.[1] The phrase 'art of thinking' had been made popular by the title of Antoine Arnauld and Pierre Nicole's *La logique, ou l'art de penser* (1662), also known as the Port Royal Logic.[2] In the hands of Baker and other authors reflecting on what it meant to be "modern" in the late seventeenth century, the phrase was used as a general name for logic or method that would apply equally well to the theories of Ramus, Bacon, Descartes, Malebranche, Locke, and even to that of Aristotle.

In this chapter, we propose an approach to the history of early modern logic and method that places our topic within the specific context of the late seventeenth-century debate over the relative worth of ancient and modern learning.[3] This debate initiated a period of "intense self-reflection" in which both the literary and the scientific merits of the present were compared with those of the past – a process which subsequently helped fashion "the narrative of Enlightenment."[4] We will show that logic, rebranded as "the art of thinking," was one of the core

[1] Baker (1699, 51). In this chapter, 'ancient' refers to classical antiquity and 'modern' to recent times. Capitalized, 'Ancient' and 'Modern' refer to authors who participated in the *querelle des Anciens et des Modernes*. The category 'early modern' is the common periodization, and 'modernity' emphasizes what historical actors considered valuable in their present.

[2] A previous occurrence (*ars bene cogitandi*) is in Gassendi (1658b,32). The Cartesians usually adopted the phrase. Gassendi also referred to logic as *ars disserendi*, which had a double pedigree: humanist-Ramist (Jardine 1974, 26, 41; Ong 1983, 178–180) and Scholastic (Petrescu 2018).

[3] Important overviews of these topics include Gilbert (1960); Jardine (1974); Jardine (1988a); Jardine (1988b); Dear (1995); Dear (1998); Nuchelmans (1998a); Nuchelmans (1998b); Nuchelmans (1998c); Serjeantson (2006); Capozzi and Roncaglia (2009); Roux (2011); Sgarbi (2013); Lu-Adler (2018).

[4] Edelstein (2010, 5 and chs. 3 and 5). See also Norman (2011, ch. 2).

concerns of authors on both sides of the debate. We will be interested in the way their reflections, whether in support of old Aristotelian or of the various brands of modern logic, contributed to the construction of an image of philosophical modernity at the high point of the Scientific Revolution. This image was inherently polemical and pluralistic, as will emerge from the variety of canons and the diversity of positions on the ingredients of the art of thinking that we will identify in their writings. Yet it will also appear that the points of disagreement reveal an underlying consensus about the core features that the logic relevant to the sciences needed to exhibit. All parties in the debate agreed that this logic would have to be the expression of the natural operations of the human mind, ground the pursuit of the sciences, and be capable of leading the mind to the discovery of genuinely novel and valuable truths about the natural world. These, we will argue, are the points on which the debaters agreed to disagree. To them, these features signaled what they found valuable about logic for their own times. The way they filled in these features, however, differed significantly from author to author.

The Art of Thinking and Philosophical Modernity

Thomas Baker, with whom we began this chapter, indicated that the late seventeenth-century *querelle des Anciens et des Modernes* was the context for his work.[5] The *querelle* has been mainly studied by historians of literature, yet it is an equally significant moment in the history of science and philosophy: the "comparisons," "parallels," and "histories" of ancient and modern learning undertaken by its participants often encompassed the whole array of disciplines and were particularly interested in assessing the new science.[6] When they did so, the chapters devoted to logic (often also called "method") served as fulcrums of the debate over the merits of ancient and modern natural philosophy. The art of thinking described the cognitive procedures apt to deliver the best results for all actions of the mind in all its endeavors, yet the crux of the debate was the art's relevance to the philosophical study of nature. While almost everyone agreed on the value of the ancients' contributions to rhetoric and poetry, the key issue that divided camps was the progress, or lack thereof, related to philosophy and the new sciences.

One early contribution to this debate came from Nicolas Poisson, the Oratorian priest and Cartesian propagandist, whose *Commentaire ou remarques sur la méthode de René Descartes* (1670) included a "parallel" (or comparison) between "vulgar" (i.e., Aristotelian-Scholastic) and "recent" logicians. Among

[5] Baker (1699, preface). [6] See Lehner and Wendt (2017).

the latter were Raymond Lull, the humanist dialecticians (Lorenzo Valla, Rudolph Agricola, Juan Luis Vives, and Petrus Ramus), Francis Bacon, Thomas Hobbes, and, above all, René Descartes, whose logic was supplemented by those of Johannes Clauberg and the Port Royal authors. All of these new logics, Poisson claimed, agreed with the "great principle" behind Descartes's method, identified as the conduct of reason and the formation of judgment.[7] Poisson's was the type of position against which the Jesuit critic and historian René Rapin, "the chief proponent of *ancienneté* in the reign of Louis XIV,"[8] reacted in his own polemical history of learning, the *Reflexions sur la philosophie ancienne et moderne* (1676). With regard to logic, Rapin's concern was to establish Aristotle's definitive contribution to the field of the "art of thinking and discoursing" and to dismiss the abortive modern attempts at reforming or discarding the Aristotelian legacy. His list of misguided moderns included Lull, the humanist dialecticians, Bacon and Descartes, as well as Girolamo Cardano, Jan Baptist Van Helmont, and Tommaso Campanella, but also the over-sophisticated Spanish Aristotelians. Instead, Rapin praised two moderns who furthered Aristotelian logic in an innovative way: Martinus Smiglecius, the Polish Jesuit, and Pierre Mousnier, the Grenoble physician who published the lectures on logic of his teacher, Honoré Fabri.[9]

The defense of the moderns took the form of encomia in the 1680s, with Charles Perrault's recitation of his poem *Le siècle de Louis le Grand* at the Académie Française (1687) and the publication of Bernard le Bovier de Fontenelle's *Digression sur les anciens et les modernes* (1687). Fontenelle argued that while the ancients were still commendable in rhetoric, history, and poetry, the moderns necessarily had the upper hand in the sciences, since the sciences were based on the *justesse du raisonnement*, exemplified best by the Cartesian *manière de raisonner*, an aptitude that was perfected over time, and thus allowed progress.[10] Fontenelle's stance was echoed by Pierre Coste, the Protestant theologian and translator, in his *Discours sur la philosophie* appended to the Cartesian Pierre Sylvain Régis's *Cours entier de philosophie* (1691), the first part of which was devoted to "Logic, or the Art of Thinking." Coste's text was written in response to Rapin's and was similarly designed as a history of philosophy.[11] Unlike Rapin, however, Coste aimed to show that

[7] Poisson (1670, 4–12). [8] Levine (1991, 268). [9] Rapin (1678, 117, 120–124).

[10] Fontenelle (1766). For Fontenelle, physics, medicine, mathematics, metaphysics, but also religion and ethics are governed by reason, and therefore allow for progress, while rhetoric, history, and poetry depend mostly on the vivacity of imagination. See also Coste (1691); Wotton (1694, 154–156).

[11] Piaia (2011, 9–11). On early modern histories of philosophy relevant to science generally, see Levitin (2015a). The relations (and extent of overlap) between early modern histories of

philosophy had reached its highest perfection in modern times. Coste limited the list of moderns to Pierre Gassendi, Descartes, and the Cartesians, among whom the relevant logicians were Nicolas Malebranche and Régis himself.[12]

In reaction to Fontenelle, Sir William Temple, the English statesman and essayist (and Jonathan Swift's patron),[13] wrote a "history of learning" meant to show that the Moderns' claims to both novelty and superiority were misguided. Temple and the other defenders of the ancients admitted there were discoveries, yet questioned the idea of progress. As Temple put it, "whether either of these be modern discoveries, or derived from old Fountains, is disputed." Similarly, Rapin commented that Descartes's principles were "not altogether new" and that Aristotle's natural historical works contained "the first hints of the most part of the new discoveries, whereof Modern Philosophy boasts."[14] Temple's main respondent was William Wotton, a classical scholar, linguist, and historian, whose *Reflections upon Ancient and Modern Learning* (1694) was commissioned by the Royal Society as a reply to Temple's *Essay*. For Wotton, logic (the "art of thinking" or "of reasoning") was "the Foundation of all Knowledge" and its key feature was its capacity of leading to discoveries in the sciences. Wotton pitted the Baconian "Advancement of real Learning" against "the Art of Disputing, and making Syllogisms" of the schools. To illustrate modern achievements, Wotton singled out Bacon, Descartes, John Locke, and E. W. Tschirnhaus.[15] Similarly short lists of luminaries meant to exemplify the superiority of non-Aristotelian over Aristotelian-Scholastic logic were put forward by Tschirnhaus himself, William Molyneux, and Jean le Clerc. These lists included Descartes, the Port Royal Logic, Malebranche, Locke, and Edme Mariotte.[16]

Wotton's work reached Gottfried Wilhelm Leibniz via Thomas Burnett, who in 1697 summarized for him the new edition of the *Reflections*.[17] In the same year, Leibniz wrote a letter to Gabriel Wagner, in which both ancients and moderns were praised for their contributions to logic. Aristotle and Porphyry, Ramus and

philosophy and of logic and between philosophical and logical sectarian groupings are topics deserving further investigation. On philosophical sects, see Bellis in this volume (ch. 5).

[12] Coste (1691).

[13] Although officially a defender of the ancients, Swift (1891, 79) derided school logic, exemplified by Aristotle and Smiglecius.

[14] Rapin (1678, 69, 174–175); Temple (1909, 25).

[15] Wotton (1694, 154–156).

[16] Le Clerc (1692, "Ad lectorem"); Molyneux (1692, "To the Illustrious Royal Society"); Tschirnhaus (1695, preface).

[17] Levine (1991, 88).

the Ramists, Cardano, Bacon, Descartes, Joachim Jungius, Vincent Placcius, and Johannes Felden – all of them had had something valuable to say about the "art of thinking," or the art of using the understanding in both discovery and judgment.[18] Leibniz's irenic goodwill toward ancients and moderns alike had its counterpart in the skeptical downgrading of both parties by Baker, whose historical account was meant to show the "Weakness of Humane Understanding" across its history, and thus the need for revelation. As far as logic was concerned, although Aristotle was to be commended for having perfected the "art of syllogizing," and Bacon for laying open "the way of free thinking," both attempts were futile, since both syllogism and induction were weak instruments of knowledge. The logics proposed by Ramus, Descartes, the Cartesians, and Tschirnhaus were equally inadequate, while the best part of the Port Royal Logic was simply borrowed from Aristotle.[19]

The various lists of modern authors of arts of thinking put forward during this debate are illustrated in Table 8.1. The table shows that, although some names appear more frequently than others, most notably Ramus, Bacon, Descartes, the Port Royal Logic, Malebranche, and Locke,[20] there was no agreement on a single canon. Alongside the plurality of canons, the table also makes clear the presence of what are, from today's perspective, both canonical and non-canonical authors. From the point of view of the early modern actors, all of them were recognizable members of the modern field of logic, whether or not they were seen as breaking with tradition. These lists are therefore apt to problematize the nineteenth-century canons that still dominate the current history of philosophy and science.

The early modern canons were the fruit of a process of historicization, since the understanding of what was "modern" was based on historical actors' attitudes toward the past. This historicizing endeavour was highly polemical on all sides, resulting in various images of what it meant to be "modern" in relation to the tradition.[21] Attitudes varied from the triumphant image of a clean break with the past (as in Perrault), to the suggestion that the modern discoveries were "derived from old Fountains" (as in Temple), through middle positions on the points of convergence and divergence between ancients and moderns (as in Leibniz). These mirror precisely the types of attitudes to the relation between the Aristotelian tradition and early modernity that the

[18] Leibniz (2006, 375, 378–379, 382). [19] Baker (1699, 51–63).
[20] On the reception of Descartes and Locke as logicians, see Schuurman (2003); Savini (2004); Ariew (2014); Pécharman (2016).
[21] The *querelle* can be fruitfully compared with the earlier debates over the *novatores* in philosophy. See Garber (2016b) and Garber in this volume (ch. 2).

Table 8.1. Canons of authors of arts of thinking

	POISSON (1670)	RAPIN (1676)	TSCHIRNHAUS (1687)	COSTE (1691)	LE CLERC (1692)	MOLYNEUX (1692)	WOTTON (1694)	LEIBNIZ (1697)	BAKER (1699)
Lull	●	●							
Valla	●	●							
Agricola	●								
Vives	●	●							
Ramus	●	●						●	●
Cardano		●						●	
Spanish Aristotelians		●							
Smiglecius		●							
Bacon	●	●					●	●	●
Descartes	●	●	●	●			●	●	●
Campanella		●							
Jungius								●	
Felden								●	
Mousnier and Fabri		●							
Van Helmont		●							
Hobbes	●								
Gassendi				●					
Clauberg	●								
Arnauld and Nicole	●		●		●	●		●	●
Malebranche			●	●	●	●			
Mariotte			●						
Tschirnhaus							●		●
Placcius								●	
Locke					●	●	●		
Régis				●					

historiography of the Scientific Revolution has explored over the last century. The latter two positions have been the preferred directions of our most recent historiography, supplanting the former perspective. The overview here suggests that these directions were already the fruit of the self-reflexive capacity of the Scientific Revolution, of which the *querelle* was one of the more visible (and noisier) manifestations.

In the rest of this chapter, we will argue that the debate over logic played an important role in the construction of an image of philosophical modernity. Certainly, since the positions in the debate reflect a specific time frame, national bias, particular ideological positions, as well as personal agendas, they can hardly be taken as the definitive face of modernity itself. Nevertheless, what we can glean from the debate is the way historical actors shaped their own perspective on a modern art of thinking relevant to science. This art would be the reflection of a "natural logic" inherent in the human mind, it would ground the pursuit of the sciences, and it would make scientific discovery possible – these are the core features both sides recognized as part of the set of philosophical values characteristic of their own time. Even when defending the ancients, authors like Rapin agreed with the defenders of the moderns on these basic features of an art of thinking. However, we will also note that these widely recognized features were diversely qualified by the individual authors and supported by different historical sources. In addition, we will show that the historical sources themselves provided a plurality of views subsumed under these features.

The Features of the Art of Thinking

For the authors on both sides of the debate, an art of thinking reflecting the natural functioning of the human mind and grounding the pursuit of the sciences with a view to discovery was at the heart of their image of their own modernity. However, there was little consensus regarding not only the sources best representing these features, but also their precise content. While seemingly unitary *mots d'ordre*, the features covered a highly heterogeneous landscape. We will explore this heterogeneity both within the debate itself and among the sources invoked by the participants in the debate.

The Focus on Mind

Reflecting their early modern sources, the participants in the debate conceived the art of thinking as an art that systematized the natural operations of the mind and directed its rules toward a useful end – in other words, as a practical,

instrumental art.[22] The art of thinking was thus premised on the existence of a "natural logic," seen as the inchoate form of the art. On both sides of the *querelle*, authors agreed that the art of thinking was simply an extension and clarification of structured procedures which the mind naturally employed in the acquisition of knowledge. An overview of this issue will nicely set the stage for our argument.

There is a double thrust to the claim about natural logic. On the one hand, it signals the importance of the operations of the mind for the grounding of logic, whether in the form of the three "Aristotelian" operations of simple apprehension, judgment, and reasoning (to which ordering is sometimes added as a fourth operation), or in that of the two Ciceronian operations of invention and judgment.[23] These models are indeed recognizable in the sources of the late seventeenth-century debate. The former structures most late Scholastic logics, as well as Gassendi's logic, the Port Royal Logic and, in its wake, all of the Cartesian logics; while the latter forms the ground of Ramus's dialectic and of Bacon's "new logic."[24]

On the other hand, the notion of natural logic rests on the identification of specific logical procedures with which man naturally is endowed. For instance, Leibniz thought that "the art of thinking must first be sought by using as models examples of good thinking," and that such examples were to be found among both ancients and moderns, whether in the form of Bacon's "art of experimenting" or in Aristotelian syllogistic.[25] Rapin took syllogism alone to be the "most consonant to the usual manner of reasoning."[26] By contrast, Coste and Poisson associated the natural manner of reasoning with Descartes's method, which "simply advises us that these rules are in us."[27] This echoes a position frequently found in their sources. For Ramus, the rules of invention and judgment were natural operations of the mind; for Bacon, the mind of man naturally followed the path of induction if it could remain untainted by "idols"; Descartes thought that intuition and deduction were activities proceeding from the innate "light of reason"; and for Locke, the

[22] For this humanist notion of "art," see Gilbert (1960, 11, 69); Jardine (1974, 41); Ong (1983, 179). Compared to the Scholastic discussions of the status of logic as either a science or an art (Campanella 1637, 2–5; Smiglecius 1638, 80–92; Mousnier and Fabri 1646, 136), here the practical nature of the art is simply assumed. See Ariew (2014, 48–50). For the Scholastic discussions of *logica naturalis*, *artificialis*, *docens*, and *utens*, see Smiglecius (1638, 92–96); Mousnier and Fabri (1646, 137); Lu-Adler (2018, 48–63).

[23] For the "Aristotelian" operations (in fact a Thomist legacy), see Rapin (1678, 118); Coste (1691); Baker (1699, 52); for the Ciceronian operations, see Leibniz (2006, 375–379).

[24] Jardine (1974, 31–34); Ariew (2014, 43–57, 157–165).

[25] Leibniz (2006, 378–379, 383).

[26] Rapin (1678, 109).

[27] Poisson (1670, 15–16); Coste (1691).

understanding had a "native Faculty" for perceiving the agreement or disagreement of ideas.[28]

A consequence of the notion of natural logic is the question about the legitimacy of artificial logic, or the *art* of thinking. The Port Royal Logic's threefold answer to this question is a good indication of early modern attitudes: one thing this art ensures is the right use of reason; a second is the knowledge and prevention of error; a third is the knowledge of our minds.[29] The first use is relevant to the focus on discovery, to which we will return below. Here we will dwell on the other two uses (in reversed order).

The art of thinking was bound up with the aim of acquiring knowledge of the human mind. This is indeed part and parcel of the sources championed by the Moderns. In *De augmentis scientiarum*, Bacon discussed logic as part of his doctrine concerning the use and objects of the faculties of the human soul; Descartes inserted an account of the human mind in the Twelfth Rule of the *Regulae*; Malebranche structured his *Recherche* according to the faculties and passions of the mind; and Locke presented his *Essay* as a natural historical investigation of the understanding.[30] The noteworthy fact here is that the scope of mind relevant to logic became wider than intellect alone; the imagination, memory, passions, as well as the body itself became involved in the art of thinking. The imagination could be relevant negatively, as the faculty at work, together with the passions and the bodily spirits, in the vices of the mind that the art of thinking was supposed to remedy (as in Bacon, Malebranche, or Locke);[31] or positively, either as the faculty partly responsible for the logical acts (as in Hobbes and Gassendi),[32] or as an aid to logical thinking (as in Descartes, Malebranche, or Tschirnhaus).[33] Memory could feature as an aid to natural history (as in Bacon), or as an aid to inference (as in Clauberg).[34] Moreover, the complex faculty of *ingenium* also became involved in the logical life of the mind.[35]

[28] Ramus (1555, 2); Bacon (1857–1874, 4:140); Locke (1975, 671); Descartes (1984–1991, 1:14, 16, 22); Descartes (1996, 10:368, 371–373, 383); Bacon (1996–, 11:197). See also Campanella (1637, 11); Mariotte (1678, 168); Régis (1691, 48); Tschirnhaus (1695, 53, 294); Malebranche (1980, 437); Arnauld and Nicole (1996, 9, 23).

[29] Arnauld and Nicole (1996, 23).

[30] Bacon (1857–1874, 4:405ff.); Locke (1975, 44); Descartes (1984–1991, 1:39–43); Descartes (1996, 10:411–417). See also Mariotte (1678, 202–232).

[31] Locke (1975, 715); Malebranche (1980, bks. 1–2, 4–5); Bacon (1996–, 11:79–109); Locke and Schuurman (2000, 237–250).

[32] Hobbes (1656, 36–37); Gassendi (1658b, 92).

[33] Tschirnhaus (1695, 280–286); Malebranche (1980, 419ff.); Descartes (1984–1991, 1:25, 43, 56); Descartes (1996, 10:388, 416, 438).

[34] Clauberg (1691, 813–814); Bacon (1996–, 11:215ff.).

[35] See Ariew (2021). On the complexities of early modern *ingenium*, see Marr, et al. (2018).

The art of thinking was also bound up with the project of guiding, regulating, and improving the human mind, a project usually designated within the *querelle* by the Cartesian label of an "art of forming judgment," adopted not only by Poisson or Coste, but also by Rapin.[36] Indeed, this definition of the aim of logic was present not only in the logics singled out by the Moderns but also in some featuring on the Ancients' lists.[37] From the perspective of the former, the key ingredient of logic as a corrective art was a specific conception of error, seen as a result of the malfunctioning or even viciousness of the faculties of the mind – an idea captured by what Poisson and Baker called the "weakness of the understanding."[38] Error in this sense could take the form of Bacon's "idols" or of Descartes's childhood prejudices,[39] and various combinations of these two models characterize the accounts of error in Clauberg, the Port Royal Logic, Mariotte, Tschirnhaus, Malebranche, and Locke.[40] In all these sources, one task of logic was to overcome error, in the sense of repairing the faulty functioning of the mental faculties. Logic thus became part of a general practice of the good life. In some places, the medical metaphor of curing the diseases of the mind (*medicina mentis*) was explicitly used and the interdependence with the medicine of the body was also thematized (as in Bacon, Clauberg, Malebranche, and Tschirnhaus).[41] Yet, while this was mostly a development in sources promoted by the Moderns, a comparable approach to error can also be found in Fabri's Aristotelian logic.[42]

In principle, this approach, which treats error as intrinsic to the mind and thus invests logic with a curative role, is in stark contrast with the traditional view of error as a breach of formal validity, and therefore as extrinsic to the mind (with the consequence that logic is seen as an instrument that aids thought, but is not part of it). However, in practice, both types of error were treated side-by-side in some non-Aristotelian logics (Hobbes, Gassendi, and

[36] Poisson (1670, 16); Rapin (1678, 98–99); Coste (1691). For the Cartesian label itself, see Cassan (2015).
[37] Smiglecius (1638, 49–58); Jungius (1641, 1); Mousnier and Fabri (1646, 50–51 , 134–135).
[38] Poisson (1670, 4); Baker (1699, 51).
[39] Descartes (1964–1974, 8A:35–37); Descartes (1984–1991, 1:218–220); Bacon (1996–, 11:79–109).
[40] Mariotte (1678, 202–225, 232–234); Clauberg (1691, 769–778); Tschirnhaus (1695, 177–183); Locke (1975, 706–719); Malebranche (1980, bk. 2, pt. 2); Arnauld and Nicole (1996, 5–9, 48–53); Locke and Schuurman (2000).
[41] Clauberg (1691, 250, 771); Tschirnhaus (1695, 217–220); Bacon (1857–1874, 3:832); Malebranche (1980, bk. 2, pt. 1); Bacon (1996–, 12:229–273). See Van Peursen (1993); Savini (2004).
[42] Mousnier and Fabri (1646, 45–48).

the Port Royal Logic).[43] This was possibly a consequence of the move toward redefining the "instrumentality" of logic – the idea of "instrument" itself was redefined to mean "intrinsic" to the mind, but for some authors the term continued to carry with it the sense of "extrinsic," too.

We have so far looked at the way both debaters and their sources attributed to logic an instrumental role relative to the human mind and noted the variety of positions behind this feature; we will now pass on to logic's relevance to the practice of the sciences and will be similarly interested in the diverse landscape of this theme.

The Focus on Scientific Practice

Rapin lamented that the new-fangled modern philosophers were "more addicted to Natural Philosophy than Logic," while Wotton openly lauded the wide applicability of the new art of reasoning, as seen in "the late inquiries into physical, metaphysical, and mathematical matters."[44] With regard to the Port Royal Logic in particular, Poisson praised its "appropriate application of the rules of logic by means of history, morals, physics, and metaphysics," while Baker complained that "Arnauld's instances are taken from other Sciences, and suppose men to be wise already."[45] Again, both sides agree on the applicability of the art of thinking, although they evaluate it differently. Two main issues are at stake here. One is the totalizing, universal nature of the art; in other words, the way it was seen to relate to the other sciences. The other has to do with the relationship between the rules of logic and the examples accompanying them.

The former issue recalls the traditional designation of logic as the "art of arts and science of sciences."[46] The idea is present among the earliest sources used in the debate. It was the hallmark of Ramus's philosophy, for instance, to turn dialectic into a super-science or method that, by dividing and ordering all knowledge with the help of the famous Ramist trees,[47] could organize an architecture of the disciplines – which in turn was the root of the encyclopedias and systems developed by later Ramists, such as Johann Heinrich Alsted, Clemens Timpler, and Bartholomäeus Keckermann.[48] The totalizing

[43] Hobbes (1656, 41–47); Gassendi (1658b, 96–97, 119–120); Arnauld and Nicole (1996, pt. 3, chs. 19–20). In the late seventeenth-century debate, at least one voice insisted on the crucial importance of formal error: Leibniz (2006, 378–379).

[44] Rapin (1678, 122); Wotton (1694, 155).

[45] Poisson (1670, 13); Baker (1699, 60).

[46] Peter of Spain's phrase (Jardine 1974, 30; Ong 1983, 182).

[47] Ramus started with methodizing the traditional liberal arts but continued to increase its scope, in the end also including religion. See Ong (1983) for an introduction to Ramus's method.

[48] Hotson (2007, ch. 4).

tendency is also visible in both Bacon and Descartes, although their grand aim was no longer the teaching and organization of knowledge, but rather its production.[49] Their respective logics were presented as the ground and sap of their trees of knowledge.[50] However, each of them was exemplified by a particular science: Bacon's art of inquiry took the form of the progress of inductive natural history from particulars to general natures, while Descartes described early on a universal science, or method, the best exemplification of which was provided by mathematics.[51] Now, whether the art was really only exemplified or in fact generated by the specific disciplines remained an unsolved question, which is also directly related to our second issue: the relationship between logical rules and examples. The question here becomes, do the examples illustrate or do they generate the rules?

The notion that the examples generate the rules was present in Ramus's dialectic and grounded the idea of natural logic. His students had to derive topical and syllogistic rules from instances of the use of reasoning found in poets, philosophers, and mathematicians. Locke's project was stronger yet – his natural history of the understanding was culled from the activity of the mind in all areas of knowledge, including the natural philosophical, the moral, and the theological. In contrast, the examples in the Cartesian logics, mostly deriving from the experimental and mathematical sciences of the day (Port Royal Logic, Malebranche, and Tschirnhaus), but also, to some extent, from the domains of rhetoric, morals, and metaphysics (Port Royal Logic), were usually illustrative of the Cartesian rules of method, even if they were also presented as contributions to the development of the sciences themselves.[52] In reply, Mariotte erased the illustrative function of the examples, which he turned into "methodological indications" that guide research in the natural sciences.[53]

In sum, for the participants in the *querelle* and the sources they referred to, the art of thinking was the fruit of a cross-fertilization between logic and the sciences that paid special attention to the practice of the sciences. Attitudes varied, however, as to the reach of logic in this process. Echoing the various positions in their sources, our authors did not reach consensus on whether

[49] Note that this totalizing tendency was not always fully realized; Fontenelle, for instance, distinguished between disciplines ruled by imagination and others ruled by reason (see note 10 above).

[50] Descartes (1964–1974, 9B:13–14); Descartes (1984–1991, 1:186); Bacon (1996–, 11:191). See also Tschirnhaus (1695, 295); Malebranche (1980, 408–409).

[51] Descartes (1964–1974, 10:374–378); Descartes (1984–1991, 1:17–19); Bacon (1996–, 11:191).

[52] Arnauld and Nicole (1996, 15–16).

[53] Roux (2011, 67–68).

logic was to be seen as the ground and substance of general systems of knowledge, or as the methodological backbone of particular sciences.

The Focus on Discovery

The question of discovery lay at the heart of the image of modernity that emerged from the *querelle*. Naturally so, since it was a debate about the value of novelty. It was also a polemical question, since it involved an evaluation of the procedures that were apt (or not) to secure discovery in the sciences.[54] However, what was meant by discovery, how it stood in relation to the ideal of demonstration, and what procedures were identified as the legitimate ones varied widely.

'Discovery' in the general vocabulary of the time might refer to the discovery of phenomena (such as the circulation of the blood) or to technological inventions (such as the telescope). In a more restricted sense, it could stand for: (a) the discovery of conclusions or theorems (by means of deduction from principles); (b) the discovery of principles (the premises of demonstrations); or (c) the discovery of arguments (the topics of the logical-rhetorical tradition). All of these senses are derived from the Aristotelian tradition, and in all cases, discovery is tightly connected with demonstration. Sense (a) is the very substance of demonstration and encapsulates the desideratum of certainty or *scientia* – a desideratum of both ancients and moderns, whether in its syllogistic or mathematical guise. Sense (b) refers to the route of discovery prior to demonstration, whereby the principles on which demonstrations depend are themselves established. This is the crux of the attack on traditional methods, insofar as they were seen to inhibit this sort of discovery. Sense (c) is mostly associated with the procedures of the dialectical *inventio* in the humanist tradition; however, Bacon used topical discovery, in a reinterpreted form, to serve the discovery of principles (which had also been the Aristotelian use of the logical topics).[55] In this way, the third sense is subsumed under the second.

Senses (b) and (a) (the discovery of principles and that of conclusions/theorems) form a tandem that was traditionally represented as the pairs of analysis–synthesis or resolution–composition. These pairings echoed

[54] Poisson (1670, 12–13); Rapin (1678, 98–99); Coste (1691); Molyneux (1692); Wotton (1694, 157); Baker (1699, 56); Temple (1909, 25–26).

[55] Bacon (1857–1874, 4:413–427); Jardine (1988b); Rossi (2013). There is also a Scholastic tradition of discussions of the topics, which informs Jungius (1641, bk. V) and Clauberg (1691, 2:844ff.); both are cited by Placcius (1689, 13), who is cited by Leibniz (2006, 376–378).

a variety of procedures, with different ancestries. They could refer to the ascending and descending routes between effects and causes in Aristotelian syllogistic (coupled with the epistemic routes between what is "better known to us" and what is "better known to nature"). Or they might evoke the mathematical procedure by which one traces backward from the posited unknown to a known principle, and then onward from the principles (axioms) to the theorems. The pairings might also reference the upward and downward routes between genera and species in Scholastic metaphysical definition; or, finally, the compositive and de-compositive moves between wholes and parts in metaphysics. In the former two cases (the syllogistic and the mathematical procedures), analysis/resolution stands for the route from particulars/effects/complexes to universals/causes/simples, and synthesis/composition for the reverse – which are the senses in which these terms were generally used in the early modern discussions of method. However, the question whether the syllogistic and the mathematical varieties of the procedures were identical or not was a matter of intense debate.[56]

Let us note now the variety of positions on the relation between discovery and demonstration and the various ways in which this pair was connected with the tandems of analysis–synthesis and invention–judgment. Some of the sources worked with a neat separation between the two stages of analysis/discovery and synthesis/demonstration (Port Royal Logic, Mariotte, Régis).[57] Sometimes discovery was seen to require a combination of analysis and synthesis, although it was itself firmly distinguished from demonstration/synthesis (Hobbes);[58] other times analysis constituted a procedure that ensured both discovery and demonstrative certainty, while synthesis disappeared from view (Malebranche, Tschirnhaus).[59] Some held that discovery should carry with it the force of demonstration, even if the procedures of invention and judgment were kept separate (Bacon);[60] others thought that analysis and synthesis served both invention and judgment (Gassendi).[61] For still others (Locke), discovery and demonstration were closely coupled in the domain of mathematics, but severed in that of natural philosophy, where discovery (of particulars rather than principles) was possible only through natural history, while demonstration was unavailable.[62]

[56] Gilbert (1960, 31–35, 81–92); Jardine (1974, 29, 39–40, 249–250); Gaukroger (2002, 74–77).
[57] Mariotte (1678, II.ii–iii); Régis (1691, 40, 54); Arnauld and Nicole (1996, pt. 4, chs. 2–3).
[58] Hobbes (1656, 48–58).
[59] Tschirnhaus (1695, 73–91); Malebranche (1980, bk. 6).
[60] Bacon (1857–1874, 4:428); Bacon (1996–, 11:97–98, 109–111, 163).
[61] Gassendi (1658b, 120–123).
[62] Locke (1975, 548–550, 643–645) and Anstey (2011b, ch. 4); but see Locke and Schuurman (2000, 234–237) and Anstey (2011b, ch. 7) for Locke's late views.

As for the procedures apt to secure discovery and demonstration in the sciences, the debate was over the authority of syllogism versus that of alternative procedures, such as the Baconian inductive natural history, the Helmontian chemical trials, or the Cartesian mathematical method. The general rhetoric against syllogism reduced it to the basis of a culture of mere disputation and speculation, without any use for discovery.[63] Yet, there were defenders of the syllogism among the advocates of both ancients and moderns. In some early modern quarters, the syllogism was conceived as a method for the discovery of principles.[64] More frequently, though, syllogism was seen to offer demonstrative power, either on its own or via mutual reinforcement with mathematical demonstration. While early in the century the certainty of mathematics was defended on the ground that it was possible to translate mathematical into syllogistic demonstration, later on the syllogism itself came to be legitimized via the idea that it encapsulated mathematical certainty and represented, as Rapin put it, the "Geometrical Method of Demonstration."[65] However, many of those who embraced the mathematical model of demonstration disengaged it from syllogistic demonstration. The ordering of one's reasoning starting from evident principles (axioms or definitions) and proceeding to certain conclusions in a linear model of deduction became the norm for Hobbes, Descartes, the Port Royal Logic, and Mariotte.[66] Yet, while Descartes repudiated the syllogism, Hobbes and the Port Royal authors still allowed it a place in their logics.[67]

Concerning the procedures for the scientific discovery of principles, we will note, again, a wide variety of positions in the sources of the *querelle*. The identification of induction as a route toward knowledge of first principles is present in various ways in the Aristotelian logical context (e.g., Smiglecius, Jungius).[68] Bacon put forward what could be called an experimental-logical analysis, in which the logical resolution was continuous with the chymical "solution" (*solutio* or *separatio*) of composites into their fundamental elements

[63] Poisson (1670, 12); Coste (1691); Molyneux (1692); Wotton (1694, 155–157) – echoing Van Helmont (1648); Bacon (1857–1874, 4:410–412, 428–429); Descartes (1964–1974, 6:17, 9B:13, 10:363); Locke (1975, 601); Descartes (1984–1991, 1:11, 119, 186).

[64] Jardine (1974, 54–58); Gaukroger (2002, 20–24).

[65] Rapin (1678, 109, 121, 193–194) – echoing Felden (1642, 3–6); Mousnier and Fabri (1646, 63, 131–132) (Fabri organizes his Aristotelian logic itself as a mathematical demonstrative science). See Gilbert (1960, 87–92); Dear (1995, 31–41) and Miller in this volume (ch. 17).

[66] Hobbes (1656, 59–64); Mariotte (1678, 181–197); Descartes (1964–1974, 10:366–370); Descartes (1984–1991, 1:13–15). Also, Arnauld and Nicole (1996, 239–240), who borrow their rules for definitions, axioms, and demonstrations from Pascal (2000, 155–156).

[67] Hobbes 1656, 33–40; Arnauld and Nicole (1996, pt. 3). See also Gassendi (1658b, 106–120). See further the literature cited in note 3 above and Anstey (2017).

[68] Clucas (2010); Sgarbi (2013, 121–126, 134–136).

(a physical variant of the decomposition of wholes into parts). This *solutio* took the form of an eliminative induction supported at every step by natural and experimental histories arranged in tables.[69] For Mariotte, the experimental and the mathematical models of analysis were combined, since observation and induction were partly conceived as problem-solving procedures that needed the aid of mathematics.[70]

The other major type of logical analysis followed the mathematical model. Of particular interest here is the notion that discovery modeled on geometry proceeds "from the simple to the complex," a phrase repeated by Rapin, Fontenelle, and Coste.[71] The source of this phrase is the third of the four rules of method formulated by Descartes in his *Discourse* – the so-called rule of order, which is preceded by the rules of evidence and division, and followed by the rule of enumeration.[72] The four rules were adopted (whether as such or augmented with material from the *Regulae*) as the substance of analysis, understood as mathematical problem-solving, by all of the Cartesian logics. Yet, different positions on the relation between the "simple" and the "complex" resulted in different accounts of analysis. On one reading, Cartesian analysis as summarized by the four rules proceeded from particulars to general principles (and was thus a variant of "orthodox" analysis, ascending from the "complex" to the "simple"), and needed to be complemented by the geometrical demonstrative method of synthesis (Port Royal Logic, Régis).[73] On a second reading, there was the idea that what was at stake was the identification of mathematical "relations" (*rapports*) or "links" between objects, the natural order of which needed to be discovered. One could do so simply by "comparing" the objects, and it was best in this sense to begin with the "simplest" objects and relations and gradually proceed to the more "complex." In this sense, analysis was self-sufficient, as it became a form of demonstration (Malebranche, Tschirnhaus).[74]

In sum, "analysis" was a fluid, sometimes equivocal notion, the fruit of an accretion of different traditions. We have also noted the plurality of views on demonstration and on the relations between discovery and demonstration. The complications of this picture were however concealed under the pervasive (sometimes triumphant) talk of "discovery," which the participants in the debate

[69] Bacon (1996–, 11:215–453; *solutio* at 254–255). [70] Mariotte (1678, 118, 124–126).
[71] Rapin (1678, 122); Coste (1691); Fontenelle (1752, 5:238).
[72] Descartes (1964–1974, 6:18–19); Descartes (1984–1991, 1:120).
[73] Arnauld and Nicole (1996, 237–238), although the authors also point out that the four rules in fact hold for all methods, not only analysis; Régis (1691, 55).
[74] Tschirnhaus (1695, 85–86); Malebranche (1980, 433, 438). See Van Peursen (1993); Lennon (2000).

saw as one key feature of the modern art of thinking. We have seen the same heterogeneity underlying the other two features, which pertain to the way the art of thinking was seen as instrumental to both the human mind and the practice of the sciences.

Conclusion

Our approach in this chapter has been to look at how the conception of a modern art of thinking was forged during a debate occurring at the turn of the seventeenth century, in which the question of modernity occupied pride of place. During this debate, the art of thinking was construed either in continuity with classical thinking or as a break with the past; but in either case, logic was enlisted in a wider discussion about the value and novelty of philosophy and the new sciences.

We have discussed the attitudes of the participants in this debate and analyzed their early modern sources by taking the perspective of the *querelle* as a starting point. This is by no means the only possible perspective on the art of thinking, of course, and it should be acknowledged that the participants in the *querelle* were biased and sometimes unreliable observers. Nevertheless, this perspective serves our central aim, which is to show how early modern logics, as variously interpreted and reinterpreted, played a role in the late seventeenth-century construction of modernity as a "philosophical" or "scientific" modernity. On a historiographical level, our approach also exemplifies the fact that any analysis is premised on a starting point that it needs to acknowledge.

The picture of the modern art of thinking was created during this key self-reflexive episode, at the apex of the Scientific Revolution, and exhibited both unity and diversity. The unity was a function of the common features we have identified in the writings of the participants in the debate: the art of thinking was seen as an expression of a "natural logic," as well as a potential guide and remedy to a malfunctioning mind; as a ground of the sciences, whether universally so or not; and as a route to discovery in the sciences. The latter two are features that we, today, generally recognize as part of our own scientific modernity, while the former is being re-explored right now in twenty-first-century terms.[75] But there was also marked diversity to the picture. This is clear from the plurality of the canons described in the first section, as well as from the heterogeneity underlying the general features of

[75] Houdé and Tzourio-Mazoyer (2003).

the art of thinking discussed in the second section. From this perspective, the early modern art of thinking was a multi-faceted, polemical construct, which reflected the multi-directional effort the historical actors undertook in order to describe their own modernity.

The Scientific Revolution itself has been interpreted, in one recent historiographical reconstruction, as a similarly dynamic, polemical, and inescapably diverse process.[76] Thus, under both its unitary and its heterogeneous aspects, our account of the early modern debate over the art of thinking mirrors and underpins this perspective on the Scientific Revolution. On a more reflexive level, concerning the relationship between the "new science" of the seventeenth century and its Aristotelian past, we notice that the main historiographical positions today seem to replicate positions already found within the early modern debate.[77] In sum, the *querelle* is a privileged historical moment, which illuminates the many facets both of the historical actors' self-understanding and of our own historiographical choices.

[76] See Garber (2016b).

[77] The historiography, especially in the history of philosophy, has tended to focus on continuities or on discontinuities, questioning or confirming the revolutionary nature of the Scientific Revolution (e.g., Barker and Ariew 1999). Another strand, especially in the history of science, has deconstructed the very idea of a Scientific Revolution (e.g., Shapin 1996).

9

Astrology, Natural Magic, and the Scientific Revolution

STEPHEN CLUCAS

Historiography of Magic and the Scientific Revolution

From the mid-1960s until the mid-1980s, any consideration of the relationship between magic and science was dominated by the so-called "Yates thesis," which held that a potent cocktail of Neoplatonic philosophy and Christian cabala fused into an occult philosophy – the "Hermetic Tradition" of Renaissance magic – which stirred up a will to operate on the natural world and an interest in mathematics, and thus contributed to the Scientific Revolution of the seventeenth century.[1] I do not propose to rehearse these themes again.[2] It suffices to say that the thesis attracted some extremely effective – albeit not always historiographically subtle – critiques from historians of science, such as Robert Westman, James McGuire, Mary Hesse, and Edward Grant,[3] so that by 1997, John Henry in his short history of the Scientific Revolution and the origins of modern science could truthfully say (despite the work of apologists such as Allen Debus and Pio Rattansi)[4] that "it is now generally agreed that the claims of Frances Yates and her followers about the influence of the so-called Hermetic Tradition have been greatly exaggerated."[5]

[1] Yates (1964, esp. ch. 8); Yates (1967).

[2] A measured appraisal can be found in Schmitt (1978), a review of Westman and McGuire (1977).

[3] Hesse (1970); Westman and McGuire (1977).

[4] See Rattansi (1973); Debus (1978).

[5] Henry (2008b, 56). While defending Hermeticism from charges that it is a "non-rationalistic philosophy," the Egyptologst Florian Ebeling concedes that Hermeticism had little to do with the development of modern science: "With the rise of modern science, however, the religious orientation of Hermeticism with its ontology of the hidden and its semiotics of symbolism, prevented it from being a part of the epistemological basis of the modern natural sciences" (Ebeling 2005; Ebeling 2007, 114).

In 1984, Brian Vickers, one of the most thoroughgoing of Yates's critics, sought to resolve the issue by insisting upon a complete non-identity between the occult and the scientific. While he observed that "the occult had a long and widely diffused influence – in parallel . . . with the non-occult sciences," and concedes that it is "essential to anyone wanting to understand the Renaissance to try to evaluate what debts, if any, the two traditions owe each other,"[6] in effect he sees the traditions as mutually exclusive. For Vickers, the relationship between the traditions was a *non*-relationship. The occult and the scientific are separate mentalities, he argued, "two traditions each having its own thought processes, its own mental categories."[7] The fact that many of what he calls "non-occult" thinkers "retain[ed] . . . occult beliefs and thought habits" is a "contradiction"[8] resolved by their "amphibious nature," which enabled them to live in two separate thought-worlds simultaneously.[9] Much of seventeenth-century thought thus had a "hybrid nature," involving the "coexistence of incompatibles."[10] While Vickers insists that "the Renaissance cannot be written in wholly modern terms," he nevertheless makes the occult/non-occult distinction his primary explanatory principle, and he is ultimately unsympathetic to the occult. It is an error, Vickers concludes, to argue "that the occult sciences in the Renaissance were productive of ideas, theories, and techniques in the new sciences."[11] Studying the occult tradition, then, is primarily of anthropological interest, as it is like studying "people of alien cultures."[12]

While it is easy to see why the Yates thesis provoked such a strong counter-reaction, any attempt at trying to think about the contribution of magic – or the occult sciences – to the Scientific Revolution (if we are to persist in essentializing the categories of the "occult" and the "scientific" in advance of our consideration) will need to take a more nuanced position, and find other ways of thinking about sixteenth- and seventeenth-century thinkers than as "amphibians" or "hybrids."

Instead of revisiting the Yates thesis and its critics and apologists, I propose to follow up on proposals by Nicholas H. Clulee and John Henry, tracing one vein of the occult tradition – radiation theory – from the Middle Ages into the seventeenth century. Vickers sees occult ideas, like celestial influences, as

[6] Vickers (1984, 6). [7] Vickers (1984, 6). [8] Vickers (1984, 13).
[9] Vickers (1984, 16).
[10] Vickers (1984, 29).
[11] Vickers (1984, 44). Vickers (2008) returns to the distinctness of science and the occult. See, in response, Newman (2009).
[12] Vickers (1984, 33, 44).

arriving at an anthropologically interesting dead end in the occult philosophy of the late sixteenth century, which was simply superseded by the emergence of a "properly" scientific worldview. But Clulee and Henry suggest a different narrative, which sees theories of radiative virtue leading to initiatives in natural philosophy that opposed late Scholasticism and embraced atomism and mechanism. This version of the story circumvents a rigid occult/scientific dichotomy, demonstrating a meaningful relationship between the two traditions.

Radial Activity and Explanations of Physical Phenomena

In his 1988 study of the natural philosophy of John Dee, Clulee disentangled Dee from his image as an "English Neoplatonist"[13] or as a "Hermetic magus"[14] by presenting a more nuanced account of his career, which demonstrated the importance, during the 1550s and early 1560s, of theories of radiative virtue deriving from the *perspectiva* tradition of the thirteenth century. Clulee traced this tradition back to its origins in the theories of the ninth-century Arabic philosopher Abu Yūsuf Ya'qūb ibn 'Isḥāq aṣ-Ṣabbāḥ al-Kindī.[15] A Latin translation of a work attributed to Al-Kindī dating from the end of the twelfth to the beginning of the thirteenth century, titled *De radiis stellarum seu theorica artium magicarum* (Concerning the rays of the stars, or a theory of the magical arts), significantly influenced scholars in the Latin West.[16]

Although the work was clearly astrological, in that it gave an account of the propagation of celestial influences, Al-Kindī extends his theory of rays to natural agents in the sublunar realm. The elemental world, Al-Kindī argues, "is a copy [*exemplum*] of the sidereal world," and thus "it is evident that each thing in the elemental world, whether it is substantial or accidental, produces (after its own fashion) rays like those of the stars."[17] All elemental things "emits rays in all directions," and "act reciprocally on one another . . . [and] are mutually patient,"[18] with different effects according to the different kinds of matter acted upon and the distance between objects.[19] Magical talismans

[13] Calder (1952). [14] See Yates (1964, 148–150); Yates (1967); Yates (1969); French (1972).
[15] Clulee (1988, ch. 2). See also Clulee (1977). On Al-Kindī, see Adamson (2007); Adamson and Pormann (2012).
[16] Al-Kindī (1975). On the date of the translation and its influence, see pp. 172–173.
[17] Al-Kindī (1975, ch. 3, p. 224). [18] Al-Kindī (1975, ch. 3, p. 224).
[19] Al-Kindī (1975, ch. 3, p. 224).

thus make use of rays to perform their effects.[20] The human mind itself also emits rays, which can affect objects in the external world. This is because, when we imagine a material thing, the species in the imaginary spirit (*spiritus ymaginarius*) acquire a corporeal existence and emit rays, which can move external things.[21] In order to bring about changes in the physical world via our imagination, all we need is a faith that the effect will be brought about. Faith (*fides*), says Al-Kindī, is the force (*robur*) and the support (*fulcimen*) of desire.[22]

When we conceive something in our spirit, we can bring it about in two ways: manually or verbally. Thus, for example, when we pray to God or to spirits, we can obtain good things or avoid bad things by means of the movements aroused by the utterance of our words, although it is often necessary to accompany words with other things.[23] Again, the power of words comes from rays: "the sounds actually produced emit rays in the same way that material things do when they act, and they can operate by means of their rays on the world of the elements in the same way as material objects can."[24] Certain sounds, especially the "names chosen by man to signify God," are particularly effective and can "modify matter by their rays, which are more effective in producing movements than the intelligence of the one who utters them."[25] "When obsecrations are addressed to God," Al-Kindī says, "provided that the desire, faith, and hope of success are present, then the words uttered obtain their effect, even if the one who utters them with a will to succeed has no faith in God."[26]

Taken as a whole, then, Al-Kindī's treatise offers a physical theory of magic – one which explains natural magic, but also incantations, talismans, and even animal sacrifice.[27] Deriving from the astrological theory of celestial influences, it extends a theory of radiative activity into the sublunar realm, as well.

Radiative Virtue and the *Perspectiva* Tradition

As Clulee, Marie-Thérèse d'Alverny, and Françoise Hudry have noted, Al-Kindī's *De radiis* – which arrived in the Latin West around the same time as

[20] Al-Kindī (1975, ch. 7, p. 251).
[21] Al-Kindī (1975, ch. 5, p. 230).
[22] Al-Kindī (1975, ch. 5, p. 232).
[23] Al-Kindī (1975, ch. 5, pp. 232–233).
[24] Al-Kindī (1975, ch. 4, p. 229).
[25] Al-Kindī (1975, ch. 4, p. 229).
[26] Al-Kindī (1975, ch. 6, p. 246).
[27] On talismans, see Al-Kindī (1975, chs. 7–8, pp. 250–254). For his theory of animal sacrifice, see Al-Kindī (1975, ch. 9, pp. 254–257).

the translation of Ibn al-Haytham's optical treatise *Kitāb al-manāzir* (under the title *Perspectiva* or *De aspectibus*), in the 1260s and 1270s[28] – was particularly important for the English natural philosophers Robert Grosseteste and Roger Bacon.[29] In his *De lineis, angulis et figuris seu fractionibus et reflexionibus radiorum* (c. 1220 to 1235),[30] Grosseteste claimed that optics, or *perspectiva*, dealt with the behavior not just of light, but of all kinds of rays, and was intended as a universal theory of natural actions.[31] Taking from Al-Kindī the basic idea that all material things emit rays in all directions, and that the rays' effects differ according to the distance they traverse and their obliquity to the objects they affect, Grosseteste develops some of the ideas already found in treatises on optics and catoptrics, particularly on the properties of burning mirrors, into general statements about how natural agents impress their effects. He then goes on to consider the effects of distance, perpendicularity, reflection, and refraction on the strength of a radiated power.

Rays are represented geometrically – by lines, angles, and figures – and without these, Grosseteste says, "it is impossible to understand natural philosophy,"[32] particularly the relationship between agents and patients, whether one is dealing with the senses or material things.[33] Here, Grosseteste introduces an important concept, which was destined to have a long afterlife in natural philosophy: the idea that force is propagated spherically. While Al-Haytham had noted in Book I of his *Perspectiva* that the radiation of light is spherical,[34] Grosseteste makes this claim for the propagation of any force or power. The sphere, he says, "is required by the multiplication of a power; for every agent multiplies its power spherically; since it does so in every direction and along all diameters."[35] This concept would later come to be known as an agent's "sphere of activity" (*sphaera*

[28] Al-Haytham (1989, lxxiii–lxxiv). See also David C. Lindberg's historical introduction to Al-Haytham (1972).

[29] The text of *De radiis* circulated in manuscripts that contained astrological works or works on optics (*perspectiva*) by Bacon and Grosseteste. See Al-Kindī (1975, 173, 174, 178); Clulee (1988, 52–56).

[30] The Latin text is available at "The Electronic Grosseteste" (www.grosseteste.org), based on Grosseteste (1912).

[31] See Grant (1974, 385): "Optics thus became for him the fundamental science of nature, revealing not only the manner of creation of the material world but also the mode of all natural actions." Also Clulee (1988, 52–53): "optics was not just the science of ray geometry but also an integral part of a natural philosophy." The extended title of an early printed version of Grosseteste's tractate emphasizes the application of mathematics to all natural agents (Grosseteste 1503).

[32] Grant (1974, 385). [33] Grant (1974, 385). [34] Al-Haytham (1989, I.3.110, p. 1:43).

[35] Grant (1974, 387). Grosseteste attributes this idea to Averroës, in the second book of *De anima* (Grant 1974, 388).

actiuitatis).[36] The other geometrical figure that Grosseteste saw as important for understanding the propagation of natural actions was the pyramid or cone. An "action is complete," he says,

> when the power of the agent comes to every point of the recipient from all points of the agent or its entire surface. But this is possible only by means of a pyramidal figure, since powers issuing from the single parts of the agent . . . converge and unite at the apex of the pyramid; and therefore all are able to act strongly on the part of the recipient encountered. Thus infinitely many pyramids are able to issue from one surface of an agent.[37]

Grosseteste's ideas about radiative force were adopted and mathematically elaborated by his younger follower, the Franciscan Roger Bacon. In his *De multiplicatione specierum* (c. 1250–1260), he describes the conical propagation of causality by means of "species" that are the "first effect of any naturally-acting thing" (*primum effectum cuiuslibet agentis naturaliter*) radiating through the universe.[38] Taking the multiplication of light as his example, since it is "more apparent to us then the multiplication of other things,"[39] he describes the geometrical propagation of force through species. Whereas Grosseteste was rather equivocal about the nature of rays or species, Bacon is more decisive: the species or rays of an agent are not material.

> A species is not a body, nor is it moved from one place to another; but what is produced [by an agent] in the first part of the air [or other medium] is . . . a likeness to itself in the second part of the air, and so on. Therefore there is no change of place, but a generation multiplied through the different parts of the medium; nor is it a body generated there, but a corporeal form that does not have dimensions of itself but is produced according to the dimensions of the air; and it is not produced by a flow from the luminous body, but by a drawing forth out of the potentiality of the matter of the air.[40]

Bacon's rather vague specifications (a corporeal form without dimensions, a drawing forth of material potential) are clearly a product of his desire to avoid a corpuscular account of the action of force. For example, the "flow" from a luminous body could be construed as an effluvium of atoms.

Al-Kindī's ideas also had an influence on the theories of natural magic promulgated by Marsilio Ficino in Book III of his *De vita libri tres* (1489), where he tries to explain how talismans can be effective, ostensibly in order to help

[36] The only study that I have been able to find devoted specifically to this important concept is Krafft (1970).
[37] Grant (1974, 388). [38] Lindberg (1983, 2–3). [39] Lindberg (1983, 92–93).
[40] Lindberg (1983, lxiii).

him interpret Book IV of Plotinus's *Enneads*. "The Arabs say that when we fashion images rightly," Ficino writes, "our spirit is joined together with the very spirit of the world and with the rays of the stars through which the world spirit acts."[41] Ficino adds that the "properties engrafted in things from the heavens" are "hidden from our senses, and hence only with difficulty known to our reason."[42] In what seems to be a direct reference to the *perspectiva* tradition of Bacon and Grosseteste, Ficino asks,

> who does not know that the occult virtues of things, which are called "specific virtues" by natural philosophers, are made not only by the elemental nature, but by the celestial? And so the rays can (as they say) imprint in images forces occult and wonderful beyond those we know, just as they introduce them into all things.[43]

He compares the action of the celestial rays to the rays of fire, but whereas heat can "illuminate, warm, dry, penetrate, rarefy, [and] melt," as we know by the evidence of our senses, these celestial rays have "much more and more wonderful powers and effects."

Although Ficino distances himself from these ideas – which he would doubt "were it not that all antiquity and all astrologers think they have a wonderful power"[44] – he notes that all astronomers agree "all of the rays of the stars penetrate in a moment the mass of the Earth," and are strongest in the center "because they touch the center perpendicularly on all sides."[45] Different stars exert different forces (*vires*), according to the difference of their rays, and from the impacts (*ictibus*) of these rays, different powers arise in elemental things, especially when the rays combine with each other.[46] Thus, at auspicious moments, "the rays of the stars quickly penetrate . . . metal and precious stones when they are engraved with images and imprint in them wonderful gifts."[47] The "celestial power" created by the "converging rays" is then retained by the matter subjected to them.

John Dee on Radiative Virtue

The ideas found in Al-Kindī, Grosseteste, Bacon, and Ficino came to singular fruition in the *Propaedeumata aphoristica* of the English mathematician and natural philosopher John Dee, first published in 1558 and reprinted in 1568. In

[41] Ficino (1989, III.20, pp. 351–353).
[42] Ficino (1989, III.12, pp. 300–301).
[43] Ficino (1989, III.12, pp. 300–301).
[44] Ficino (1989, III.15, pp. 318–321).
[45] Ficino (1989, III.15, pp. 320–321).
[46] Ficino (1989, III.16, pp. 324–325).
[47] Ficino (1989, III.16, pp. 322–323).

the opening aphorisms of Dee's work, the basic tenets of radiated force are laid out, reading almost like an executive summary of the medieval writers, whose works Dee owned in manuscript.[48] Dee sets out a theory of astral influences based on the geometrical models of the *perspectiva* tradition, and his aim is to use his knowledge of the ways celestial virtues are propagated in order to reach magical effects.

I do not have space to elaborate, but I will single out his intention to use his knowledge of catoptrics – i.e., mirrors – to focus, multiply, and impress the rays of the stars and planets. Dee had a quite sophisticated understanding of catoptrics, and wrote a whole treatise on burning mirrors, particularly the parabolic burning mirror, which was reputed to be the optimal form for such devices.[49] If a burning mirror allows the user to multiply the rays of the Sun in order to augment its natural power, why should it not be possible, Dee thought, to do the same for other celestial bodies, which also radiate their influences upon Earth? Thus, in Aphorism 52, Dee tells his readers:

> If you were skilled in catoptrics, you would be able by art, to imprint the ray of any star much more strongly upon any matter subjected to it than nature itself does. This indeed was by far the largest part of the natural philosophy of the ancient wise men.[50]

In the following aphorisms (53 and 54), Dee exhorts the "diligent investigator of secrets" to make use of this "mode of experiencing [or experimenting]" (*experiendi modum*) to test the use of mirrors with the full moon, and the "peculiar powers not merely of stars but also of other things which they work upon through their sensible rays."[51] Finally, in Aphorism 89, he claims that:

> By skill in catoptrics, however, you will cause any one of the five planets to stand off very far from the Earth; and that within the space of a few days. And finally, in the winking of an eye, you may be able to draw it, as it were, to a new perigee.[52]

What Dee means here, I think, is not literally moving the planets about, but artfully deflecting, mitigating, or amplifying the astral influences emanating from them.[53]

[48] See especially Dee (1978, Aphorisms 4–7).
[49] The efficacy of the parabolic mirror had been known since Diocles' treatise on burning mirrors of 3 BC. See Knorr (1983).
[50] Dee (1978, 148–149). [51] Dee (1978, 148–149). [52] Dee (1978, 174–175).
[53] See also Dee's *De speculis comburentibus*, British Library MS Cotton Vitellius C. VII, fols. 279–309, bk. II, prop. 65.

The Physics of Radiated Virtue

So, one trajectory for this cluster of ideas about rays and the propagation of forces results in a set of ideas that Vickers would have no difficulty labelling 'occult'. Both Ficino and Dee – key figures for the Yates thesis – used radiation theory to underpin magical ideas about talismans, and the harnessing of celestial influences.

There is, however, another way of thinking about theories of radiated virtue. As Clulee rightly observed, the "physics of radiated influence," like the "naturalistic Aristotelianism" of Nicolas Oresme and Pietro Pomponazzi, "promoted an understanding of magical and occult phenomena as natural and rationally intelligible."[54] The ideas transmitted in the *perspectiva* tradition, Clulee says, were extended to a "broad range of causal relationships," provided "an intelligible and non-spiritual, non-demonic treatment of the action of 'occult' qualities and influences," and also facilitated "the extension of mathematical treatment to domains of physics beyond those Aristotle had considered appropriate for mathematical study."[55] Clulee saw Dee as a case showing that different aspects of magic, or the occult tradition, had "different implications for science." He saw Dee's use of ritual magic traditions in his angelic conversations as having a "predominantly religious function," which was "quite separate from his use of the mediaeval tradition of . . . natural magic."[56] In a move which skews away from the Yates thesis, Clulee concluded:

> If a sense of operational power, a curiosity to test the secrets of the occult tradition, a willingness to consider the occult as intelligible and a confidence in finding explanations for insensible agents were ways that Renaissance magic prepared the ground for seventeenth-century science, these were more a central feature of natural magic, independent of Hermetism, Neoplatonism, and Kabbalah, than of the more religiously motivated ideas of magic.[57]

While one could quibble with the idea of medieval magic being independent of Neoplatonism (there are Platonic colorings in Al-Kindī's *De radiis*, and Ficino gives a Platonic interpretation of stellar radiation), I believe this thesis is broadly correct. If we want to look for the ways magic contributed to the Scientific Revolution, we need to look toward natural magic, rather than the ritual and ceremonial magic that also enjoyed a wide circulation and influence between the thirteenth and seventeenth centuries.

[54] Clulee (1988, 239–240). [55] Clulee (1988, 233). [56] Clulee (1988, 240).
[57] Clulee (1988, 241).

In Chapter 4 of his *The Scientific Revolution and the Origins of Modern Science* (1997), "Magic and the Origins of Modern Science," Henry argues for the importance of magic on a number of fronts.[58] Like Clulee, he argues that Renaissance thinkers such as Pomponazzi "refined the more naturalistic aspects of the magical tradition." For those wanting to "understand the role of magic in the Scientific Revolution," he says, "natural magic is arguably the dominant aspect of the magical tradition."[59] Actually, he goes further, and suggests that "the separation of the naturalistic elements from other aspects of magic was just what was accomplished during the Scientific Revolution,"[60] and that "the scientific worldview developed, at least in part, out of a wedding of natural philosophy with the pragmatic and empirical tradition of natural magic."[61] Henry sees the new emphasis on the "occult powers capable of acting upon some or all other bodies" as one of the most important contributions of natural magic.[62] These "occult qualities" included astral influences and magnetism.[63]

While the Aristotelian tradition saw occult qualities as unknowns that could not be explained by natural philosophy, natural magic showed itself willing to make these unknown powers or qualities intelligible. Henry here alludes briefly to an argument in his important 1986 article on occult qualities in pre-Newtonian matter theory,[64] where he shows the importance of active principles in seventeenth-century natural philosophy, including Walter Warner's early seventeenth-century manuscript writings on atomism, which invokes *vis radiativa* or "vertue radiative," the "efficient power or vertue" in all bodies.[65] I have elsewhere discussed at length the atomistic theories developed in the so-called "Northumberland circle" of the late sixteenth and early seventeenth century (including Warner and his colleague Thomas Harriot), which made use of "radiative virtue" as an explanatory principle for various kinds of atomic change.[66] So here I will focus on two mid-seventeenth-century natural philosophers – Kenelm Digby and Walter Charleton – who also draw on the radiation theories of the *perspectiva*

[58] Henry (2008b, 56–68).
[59] Henry (2008b, 56). See also Henry (2008a). Both Clulee and Henry's references to Pomponazzi point to *De naturalium effectuum causis, sive de incantationibus* (1556), which sought to provide naturalistic explanations for a number of magical and miraculous phenomena. See the introduction to Pomponazzi (2011, xi–lxxi).
[60] Henry (2008b, 57). [61] Henry (2008b, 57). [62] Henry (2008b, 65).
[63] The existence of astral radiation which influenced the sublunar world was widely accepted in medieval natural philosophy. See North (1986); Grant (1987b); Weill-Parot (2010); Christie (2019).
[64] Henry (1986). See also Hutchison (1982); Blum (1992); Fleming (2011).
[65] Henry (1986, 340). [66] See Clucas (2001).

tradition in elaborating their natural philosophies. In what follows, I show how rays and the spherical propagation of forces informed natural philosophy aside from John Dee's astral magic.

Charleton and Digby both possessed many of the credentials we associate with the "new science" of the seventeenth century. They are anti-Aristotelian and highly critical of late-Scholastic appeals to abstractions, such as forms and qualities. They offer instead corpuscular theories, which explain natural processes in terms of corporeal actions. They are admirers of Galileo and Mersenne, but they also use concepts drawn from the tradition of radiative virtue. Crucially, however, both philosophers redefine rays in corporeal, atomistic terms, shifting away from the immaterial species favored by Roger Bacon.

Walter Charleton

In his 1650 translation of a work on the magnetic cure of wounds by Jan Baptist van Helmont, Walter Charleton espoused a doctrine of radial activity. In his "Prolegomena to the Candid and Ingenuous Reader," Charleton wrote:

> I am bound to believe, that in the infinite Magazine of Nature are to be found various Agents. Not obliged to the dull conditions of an immediate *Corporeal Contact*; but richly endowed with an *Influential* or *Radial Activity* (so I have taken the liberty to Christen it), in reference to the cognation and near Analogy, betwixt the manner of the Stars transmitting their influence, by invisible Deradiations; and the imperceptible Emissions streaming in a semi-material thread of Atomes; from sublunary bodies . . . .[67]

Charleton may in part have been indebted to the radiation theories of Van Helmont himself, although radiation theory makes only a fleeting appearance in the latter's natural philosophy.[68] Van Helmont criticized the late Scholastics for neglecting natural phenomena involving action-at-a-distance, which he saw as the effect of a celestially infused agent he calls *Blas*:[69]

> For truly, besides the actions of the Heavens (which are carried by influence, in-beaming, and motion, without the touching of an Agent; but by a Blas onely do disperse the Seminaries of their own virtues) Sublunary things are not properly deprived of a Blas: Because fermental Odours, do produce most active, and seminal effects, and do transchange, in nature, their object, by

[67] Charleton (1650, sig. D1^{v}–D2^{r}). On Charleton and radiative virtue see also Henry (1986, 340–341).

[68] Radiation theory is not mentioned, for example, in the most comprehensive account of Van Helmont's natural philosophy to date (Hedesan 2016).

[69] On Blas, see Hedesan (2016, 80–83).

> their own perfume, and do draw it after them into their protection. Likewise also a radial or beaming action doth concur into nature.[70]

Seminal effects are conceived by Van Helmont as "beaming actions" (*actiones radiales*), although unlike the radiated forces in the *perspectiva* tradition, there is no reciprocity between agent and patient in Van Helmont's theory.[71]

Charleton's radiation theory is much more elaborate, and more corporeal. In his *Physiologia Epicuro-Gassendo-Charltoniana* (1654), Charleton used his theory of "Influential or Radial Activity" to explain a number of phenomena that had been discarded by the Scholastics as unknowable occult qualities.[72] He compares the action of the "tenacious Rayes . . . abduced from Amber and other Electriques," for example, to a chameleon's tongue darting out to draw an insect into its mouth. These rays, however, are not immaterial species, but streams of atoms: "swarms of subtle Emanations, or Continued Rayes of exile particles" whose "radial Diffusion" captures other bodies and "hold[s] them fast in their Complicate embraces." While attractive forces like static electricity and magnetism had long been associated with occult virtues or qualities, Charleton reinterprets their insensible activity. They are "corporeal" rays, not "Immaterial" or "Supernatural" virtue.

Like Al-Kindī, Charleton sees these radial diffusions as a universal property of bodies: "most, if not All Bodies," he says, "emit Effluvias from themselves to others." These "Aporreas or Effluxes streaming from bodies" are of "great Exility," but are nonetheless corporeal.[73] Even light is construed as a body, rather than a "meer Quality" as the Peripatetics suggest.[74] "By the Rayes of Light," he says,

> we understand, certain most tenuious streams of Igneous particles, in a continued fluor, and with ineffable pernicity succeeding each other in direct lines, either immediately from their lucid Fountain, or mediately from solid bodies reflecting them, towards the eye, and sensibly affecting the same.[75]

These streams of igneous particles are propagated spherically from the luminous body, which is

[70] Van Helmont (1664, 335).
[71] Van Helmont (1664, 330): "an Agent suffers not a whit by reaction, in seminal, or beaming actions, and by consequence, that neither doth the Patient therefore re-act." See also Van Helmont (1652, 336).
[72] Charleton (1654, 342). [73] Charleton (1654, 344). [74] Charleton (1654, 204–205).
[75] Charleton (1654, 198).

> the Center from which all its luminous Rayes are emitted by Diffusion Sphaerical according to that establisht maxime of Alhazen [i.e., Al-Haytham], *Omne punctum luminosum radiare sphaeraliter* . . . we may [also] understand it to emit rayes in a diffusion Pyramidal, the base whereof is in it self, and the cone in the eye of the Spectator.[76]

Magnets, too, emanate "magnetique Rayes," which are "diffused from the Centre of the Loadstone to all points of its superfice, and beyond it to the bounds of their orb of activity."[77] The "*species*" of the Loadstone are "transmitted by way of Radiation" and

> every Ray is Tense and Direct in its progress through the intermediate space, like a small thread of wire extended, and this because . . . the praecedent particles are still urged and protruded forward in a direct line, by the consequent, after the same manner as the rayes of light.[78]

Unlike the ambiguously defined species and rays of the *perspectiva* tradition, however, these are fully material, comparable to "Nerves and Lutestrings"[79] or the tongues of chameleons.

Kenelm Digby

Like Charleton, Digby makes extensive use of radiation theory in his natural philosophy. In his treatise on the nature of bodies, published in 1644, he notes – following in the footsteps of William Gilbert – that magnets have a "sphere . . . of operation" or "activity," which consists of "emanations."[80] These emanations "issue out and send its streames abroad; in such sort as it is the nature of all bodies to do."[81] The magnetic sphere of activity, that is to say, is not a special case of natural phenomena, but is true of all bodies. "[N]o particular body," Digby says, "can be exempt from the lawes of all bodies: and that we haue aboue declared that euery physicall body must of necessity have

[76] Charleton (1654, 202). Charleton may have been thinking of the *Perspectiva* of John Pecham, rather than Al-Haytham, here. See Pecham (1542, prop. 6, sig. biij^{v}): "Omne punctum luminosi hemisphaeraliter super medium radiare."

[77] Charleton (1654, 391). Also: "the Magnetique Species being diffused by Deradiation Excentrical, and the Attraction of the Loadstone (of a spherical figure) being thus circumradious, or from all points of the circumference of its sphere of Energy: it will be requisite that we allow it to have (1) a *Centre* . . . (2) an *Axis* as that to which the virtues of all the circumjacent Fibres are contributed, (3) the *Diametre of an Aequator*, which lying in the middle of all its Fibres, may also contain the strongest virtue of them all" (Charleton 1654, 391).

[78] Charleton (1654, 390).

[79] Charleton (1654, 345).

[80] In *De magnete*, Gilbert described magnetic attraction as an *orbis virtutis* (orb or sphere of power). See Gilbert (1600, sig. v^{r}). For his use of the concept see, e.g., Gilbert (1600, 83–84, 103).

[81] Digby (1644, XXI.3, p. 187).

an orbe of fluours, or a sphere of actiuity about it."[82] Again like Charleton, Digby materializes the species of the *perspectiva* tradition, rejecting the immaterial species of the traditional view, although he retains the ray and the sphere of activity which derive from that theory. "[T]hose masters who will teach vs that the impressions vpon sense are made by spirituall or spiritelike thinges or qualities," Digby says,

> which they call intentionall specieses [*sic*], must labour att two workes: the one to make it appeare that there are in nature such thinges as they would perswade vs; the other, to proue that these materiall actions we speake of are not able to be performe those effects . . . .[83]

Digby, like Charleton, saw light and other radiated virtues as corporeal.[84] Yet he continually refers to the "orb" and "sphere" of corporeal effluvia, and states that "there is not a body in the world, but hath about it selfe an orbe of emanations of the same nature which that body is of."[85] When discussing the question, "What is the sphere of actiuity in corporeall Agents," Digby says that what philosophers mean by this can be explained by looking at what he has to say about fire and its "manner of operation."[86] He describes his conception of fire as follows:

> if in our fantasie, we frame an image of a round body all of fire; wee must withall presently conceiue, that the flame proceeding from it, would diffuse it selfe euery way indifferently in straight lines; in such sort, that the source seruing for the center, there would be round about it an huge sphere of fire and of light; vnlesse some accidentall and externe cause, should determine its motion more to one part then to an other. Which compasse, because it is round, and hath the figure of a sphere, is by Philosophers termed the sphere of its actiuity.[87]

The motion of fire, he says, is "fluxe in a direct line from the center of it, to its circumference."[88] This model for Digby explains all interactions between bodies, even that of gravity. "[I]f any man desireth to looke vpon . . . the whole chaine of this doctrine of grauity," Digby writes,

> lett him turne the first cast of his eyes vpon what we haue said of fire when we explicated the nature of it. To witt, that it beginneth from a little source;

[82] Digby (1644, XXI.6, p. 189). [83] Digby (1644, XXVII.3, pp. 244–245).
[84] On the corporeality of light, see Digby (1644, VI.3–7, pp. 41–45).
[85] Digby (1644, XVI.1, p. 138). [86] Digby (1644, XVI.1, p. 137).
[87] Digby (1644, V.8, p. 38). See Bacon (1626, I.31, p. 11): "the forme of a Piramis in Flame, which we vsually see, is meerely by Accident . . . For of it selfe it would bee Round."
[88] Digby (1644, V.8, p. 38).

> and by extreme multiplication and rarefaction, it extendeth it selfe into a great sphere.[89]

What is important in the cases of both Charleton and Digby is that the concepts borrowed from the *perspectiva* tradition, once reconceptualized as flows and emanations of corporeal bodies, offer them a viable and, in their view, more intelligible alternative to Aristotelian abstractions. When the fourteenth-century philosopher Richard Swineshead attempted to explain hot and cold bodies, Digby says, he ran into an "insoluble" argument. This, Digby explains, is because Swineshead seeks to explain these phenomena using the "doctrine of qualities," which is unable to "giue account of the ordinary and plaine effects of nature."[90] Digby is equally critical of the no less abstract medieval concept of the intension and remission of forms (*intensio et remissio formarum*).[91] The "latter sectatours, or rather pretenders of Aristotle" have been guilty of introducing needless sophistications. There is no need of Aristotle's doctrines of "*formes* . . . and *priuations*." We can explain natural phenomena simply by looking at "the actuall composition of compounded thinges" and their "particular motions."[92] What was needed, however, was a mechanism that explained how one body acted on another, and this explanatory lacuna was filled by radiation theory.

The history of this theory did not, of course, end with Digby and Charleton, or indeed with the seventeenth century. One very concrete possibility is that the radiative tradition may have made it possible for Isaac Newton to think about forms of material interaction in his natural philosophy that did not involve direct contact between agent and patient. There has been lengthy discussion in Newton scholarship about how precisely Newton conceived of gravitational force. John Henry has looked at the "pre-history" of Newton's physical theories, and the possible influence of "active principles" in the century preceding publication of his *Principia*.[93] Newton himself prevaricated about how exactly gravitation worked, and whether or not it required active or subtle matter or an immaterial substance (such as God). Nonetheless his speculations in the queries appended to the *Opticks* suggest that he had considered the possibility that physical causation is radiative. In Query 29, for example, Newton speculated that the reciprocal

[89] Digby (1644, X.5, p. 79). [90] Digby (1644, XVI.5, p. 141).
[91] Digby (1644, XVI.6, pp. 141–142). [92] Digby (1644, "Conclusion" to Treatise 1, p. 344).
[93] See Henry (1986); Henry (2011). See also Ducheyne (2011); Schliesser (2011); Wang (2019, esp. 73–83).

action of light rays and "pellucid substances" could offer a model for the way in which attractive forces act on remote bodies (e.g., gravitation):

> Pellucid Substances act upon the Rays of Light at a distance in refracting, reflecting, and inflecting them, and the Rays mutually agitate the Parts of those Substances at a distance for heating them; and this Action and Reaction at a distance very much resembles an attractive Force between Bodies.[94]

In Query 31, Newton went further and declared – in a way reminiscent of Walter Warner's use of *vis radiativa* in a corpuscular context – that all corpuscles had the power to act on each other at a distance. "Have not the small Particles of Bodies," he asked, "certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the Rays of Light . . . but also upon one another for producing a great Part of the Phaenomena of Nature?"[95] Although he declines to speculate "How these Attractions may be perform'd," it is clear that he saw the action and reaction of rays at a distance as a plausible model.[96] Since emanating virtues were still a viable theoretical option in the late seventeenth century, Newton was able to resist calls to reduce all physical interactions to instances of contact and collision.

Conclusion

What I hope to have shown in this brief survey is that there was a relationship between part of the occult tradition and the Scientific Revolution. While it has nothing do with ritual magic, or Hermeticism, radiation theory nonetheless begins life as a theory of magic – albeit a physical one – and a theory predicated on the idea of a harmony existing between the celestial and sublunary worlds. Al-Kindī's extension of the ray theory of celestial influence to natural bodies in the elemental world opened the way for a more general consideration of natural agents and forces in terms of radiation. Natural magic provided ways to make occult qualities and insensible agents intelligible and explicable, and the isolation of the powerful explanatory models provided by radiation theory during the seventeenth century was an important part of what made the Scientific Revolution possible. The concept of force was one of the cornerstones of the "new science" of the seventeenth

[94] Newton (1952, 370–371).
[95] Newton (1952, 375–376). On Warner's use of *vis radiativa* as an active principle in a corpuscular context, see Clucas (2001, 183–196).
[96] Newton (1952, 376).

century and so-called "occult" ideas were an important part of its development.[97] In the second half of the seventeenth century, when natural philosophers were moving beyond the idea of mechanical collision as a universal causal explanation for natural phenomena, radiative force was able to supply a possible alternative. In view of the longevity of the radiative tradition, and its continued significance in non-Aristotelian natural philosophies in the seventeenth century, I would argue that any meaningful attempt at establishing the philosophical foundations of the Scientific Revolution must include a consideration of natural magic and the "occult sciences."

[97] See, e.g., Gabbey (1971); Westfall (1971b); Manchak (2009).

10

Practitioners' Knowledge

JOEL A. KLEIN

Historiography: Theory and Practice, Art and Nature

The status of the knowledge of early modern artisans and craftspersons, subsumed here under the term 'practitioners' knowledge', has been approached from multiple scholarly perspectives and has been a point of contention throughout the history of philosophy and scholarship on the history of science. In recent historiography, the question of the extent to which these practitioners influenced the emergence of science has been shaped by divergent understandings of the nature of the Scientific Revolution and the relationship between theory and practice. In short, many scholars favor the view that the history of science is predominantly about theoretical science, and is generally the domain of intellectual history. These scholars hold that early modern artisans and practitioners – who were generally unlettered – were unimportant or, at best, tangentially related to the Scientific Revolution, which they regard as a series of events taking place primarily in the seventeenth century. Other scholars increasingly have challenged this view and have argued that the experimental aspects of science must be comprehended alongside theory. Some authors further conclude that artisans and practitioners were central to the emergence of the experimental science, and this, in turn, has suggested a continuity with the Middle Ages that entails a broader historiographical scope. Other debates concern whether there is a meaningful difference between practitioners' knowledge and theoretical knowledge, or, similarly, if scientific work is best understood in terms of epistemic activities and systems of practice instead of the more customary understanding of knowledge based on propositions.

An early and prominent advocate of the view that craft practitioners and artisans were irrelevant to the history of science was the nineteenth-century

British polymath William Whewell, who famously coined the term 'scientist'. Whewell proclaimed that the work of the "master artists of the middle ages," responsible for, *inter alia*, magnificent pieces of visual art or towering cathedrals, was "inadmissible . . . into the series of events which mark the advance of science."[1] Whewell reasoned that such individuals, while they may have had a functional knowledge of mechanics or geometry based on the "practical assumption of a principle," lacked "speculative knowledge," and therefore could not contribute to science.[2]

In the early twentieth century, when history of science was being established as a formal discipline, some historians arrived at conclusions about the influence of artisans that swung in the opposite direction. One group of historians, some of them Marxists, claimed that artisans were central to the rise of modern science.[3] Among these scholars, the Austrian-American historian and Marxist Edgar Zilsel is especially notable. In an influential article published in 1941, Zilsel argued that the phenomenon of early modern science is best understood as a sociological process marked by developments in technology and changing appraisals of the importance of quantification.[4] He claimed that "science was born" in the sixteenth century, after "superior manual laborers" – a group distinct from university scholars and humanists, and among whom he included "artist-engineers, surgeons, the makers of nautical and musical instruments, surveyors, navigators, [and] gunners" – developed methods of experimentation and quantification that were eventually co-opted and adopted by scholars.[5]

Other scholars argued in favor of the importance of practitioners in the history of science, but focused more precisely on the history of technology and commerce. Reijer Hooykaas argued that the emancipation of the burghers in the Renaissance was crucial to the ennoblement of manual labor and allowed artisans and scholars to cooperate, leading to "a rapid development and refinement of the experimental method."[6] In *Science, Technology and Society in Seventeenth-Century England*, Robert K. Merton traced some of the larger "cultural values" that underlie modern science, stressing socio-

[1] Whewell (1837, 334).
[2] Indeed, Whewell compared this base practical thought to the behavior of brute animals and even marshaled evidence for this comparison from Proclus's account of Epicureans, who claimed that an animal such as an ass "knew that two sides of a triangle are greater than the third" (Whewell 1837, 334).
[3] For an extended discussion of this twentieth-century historiography, see Long (2011, ch. 1).
[4] Zilsel (1942).
[5] Zilsel (1942). For a brief summary of critiques of the "Zilsel Thesis," see Cormack (2017).
[6] Hooykaas (1972, 92).

economic and utilitarian factors in marine transport and navigation, mining and metallurgy, military technology, textile production, and husbandry, all of which he suggested were critical to the development of early modern science.[7] Charles Webster's *The Great Instauration* likewise spotlighted the development of technology in England and traced a history of empirical and experimental natural philosophy that emphasized medicine and practical and utilitarian concerns in the broader social and economic reforms of the seventeenth century.[8]

Still, many historians and philosophers of science in the Anglo-American tradition rejected this emphasis on craft and artistic knowledge, and hewed closer to the line established by Whewell and others. Alexandre Koyré was especially influential in this regard, and drew a strict distinction between theory and practice, going so far as to suggest that Galileo Galilei had arrived at a science of falling bodies not through experiments – which Koyré claimed Galileo lacked the technology to perform – but strictly through philosophical deduction.[9] Another scion of this internalist tradition, A. Rupert Hall, suggested a hierarchical relationship between scholars and craftsmen, wherein the latter merely supplied the "raw material" with which scholars wrought the changes central to the Scientific Revolution. Hall argued, moreover, that "The great discoveries of mathematical physicists were not merely over the heads of practical engineers and craftsmen; they were useless to them."[10] More recently, H. Floris Cohen cited both Koyré and Hall in his conclusion that arts, crafts, and technological advances influenced the development of early modern science, but only insofar as these provided "food for thought" to philosophers and scholars. The work of artisans and craftspeople thus may have helped to direct the course of the Scientific Revolution, but Cohen is explicit that such work "did not . . . implicitly contain that revolution."[11] Similarly, in an influential article, Thomas Kuhn sharply differentiated between mathematical and experimental traditions in the Scientific Revolution, arguing that the latter – which included craft practices as well as Baconian experimentalism – was only tangentially related to the main developments, which were primarily "a revolution of ideas" in "traditional, quasi-mathematical fields."[12]

[7] Merton (1938, xxxi). [8] Webster (1975).
[9] Thomas Settle challenged Koyré's conclusion by reconstructing Galileo's experiments (Settle 1961).
[10] Hall (1959, 21). [11] Cohen (1994, 350).
[12] Kuhn (1976, 10). Several authors have challenged Kuhn's thesis. See Bennett (1986); Pastorino (2011). For a historicized account of debates between mathematicians and naturalists in the post-Newton Royal Society, see Feingold (2001).

A burgeoning group of scholars began to question this prevailing, internalist understanding of the Scientific Revolution during the final decades of the twentieth century.[13] In this vein, some also revisited traditional distinctions between theory and practice, a turn that has been a part of a larger movement toward constructivist historiography in which scientific knowledge is taken to be produced by human culture rather than by a progression of theories and ideas.[14] Scholars have pointed to decidedly non-revolutionary continuities between the Middle Ages and the early modern era, and likewise have problematized the concept of science as a monolith founded primarily upon the history of ideas of great European men. This has extensively widened the scope of the history of science and incorporated not only histories of artisans, but also disciplines that were long written off as pseudoscience, such as alchemy and astrology.[15]

The constructivist approach has also contributed to the emergence of practice, materiality, and socio-cultural negotiations as essential foci for understanding the development of science. This has impelled scholars to explore areas – both topical and geographical – that have been largely neglected in traditional histories. In addition to work on artisanal knowledge and disciplines dismissed as pseudoscience, there have been important studies on practices of archiving and collecting that were integral to science, the contributions of early modern midwives and other women engaged in producing natural knowledge, and diverse other "invisible" technicians whose work in the laboratory or workshop was often forgotten or co-opted. Likewise, striving for a more global perspective, historians have turned their attention, for example, to the commercial trade of goods, modes of information exchange, the relationship between science and the administrative state, and the production and transmission of visual and material culture.[16]

Following these trends – and helping to set them – historians have revisited earlier theses about the influence of artisans, but ultimately have advanced a more historicized and less programmatic understanding of early modern science. Paolo Rossi focused on artists and practitioners in England who influenced Francis Bacon, and he argues that such practitioners transferred

[13] As Steven Shapin put it in the opening sentence of his survey of the Scientific Revolution, "There is no such thing as the Scientific Revolution and this is a book about it" (Shapin 1996, 1).
[14] Golinski (2005).
[15] For recent historiography on alchemy, see Moran, et al. (2011). On astrology, see Rutkin (2006).
[16] For an overview of these historiographical changes, see Smith (2009).

certain fundamental values to the practice of science. For example, understanding how to make an object like a mechanical device provides a type of knowledge aside from traditional conceptions of philosophical knowledge – i.e., Aristotelian understandings of causes.[17] Rossi also showed that Bacon owed a significant debt to a sixteenth-century revival of interest in the mechanical arts.[18] James Bennett has also written extensively on the mechanical arts, but has noted the importance of practical mathematics and instrument builders in the history of science, and Eric Ash has explored how "expert mediators" were instrumental in processes of state building through practical and theoretical knowledge of such technological processes as copper mining, navigation, and harbor construction.[19] In *The Jewel House: Elizabethan London and the Scientific Revolution*, Deborah Harkness provides an ethnographically informed and locally situated account of a diverse set of sixteenth- and seventeenth-century Londoners, including alchemists, craftsmen, merchants, medical practitioners, instrument makers, and many others united by an interest in what she calls "vernacular science."[20] Similarly, William Eamon has explored the codification of artisanal and craft "secrets" in so-called "books of secrets" such as Alessio Piemontese's influential and oft-translated *Secreti*, and a number of other scholars have written exemplary studies of early modern recipe literature and of domestic science and medicine.[21]

In the last two decades, the two scholars who have written most directly and extensively on the knowledge of artisans and practitioners are Pamela O. Long and Pamela H. Smith. In *Openness, Secrecy, Authorship: Technical Arts and the Culture of Knowledge from Antiquity to the Renaissance*, Long explored the possession and transmission of knowledge through a survey of diverse technical treatises, ultimately demonstrating how writing on the mechanical arts became an integral component of early modern science.[22] In *Artisan/Practitioners and the Rise of the New Sciences, 1400–1600*, Long borrows Peter Galison's concept of "trading zones" to suggest a means by which information central to the development of new attitudes toward nature was exchanged between scholars and artisans.[23] Long problematizes familiar distinctions between "scholar" and "craftsman," suggesting that modern definitions of these terms are ill-suited to the complexity of early modern social and cultural interactions. Similarly, Long traces the shifting

[17] Rossi (1970). [18] Rossi (2013). [19] Ash (2004). [20] Harkness (2007).
[21] Eamon (1994); Rankin (2013); Leong (2018). Also see the collection of essays in Leong and Rankin (2011).
[22] Long (2001). [23] Long (2011).

relationship between "art" and "nature," suggesting that in the late Middle Ages, the arts took on greater prestige at the same time that the concept of art was changing from the inferior mimesis of nature to the perfection or completion of nature. As artisanal and learned culture drew nearer and overlapped, so too did the understanding of art and nature. Following medieval alchemy, perhaps the most notable of the perfective arts, and driven by the early modern expansion of the interest in consumer and luxury goods, a broader movement arose in the fifteenth and sixteenth centuries that viewed nature as something that could be controlled and manipulated.[24]

In *The Body of the Artisan: Art and Experience in the Scientific Revolution*, Smith has argued that early modern artisans and craftspeople developed a way of thinking about and engaging with the natural world centered upon practice, and which was central to the rise of the new experimental philosophy and its subsequent institutionalization.[25] Smith calls this philosophy an "artisanal epistemology," and defines it as "a bodily form of cognition connected to a view that matter and nature are alive and exist in synergy with the human body."[26] This philosophy was expressed primarily in the production of things, but occasionally also in texts, as in books of secrets or recipe collections. These art objects and writings were produced from a "bodily engagement with matter" and a "bodily struggle" with nature in which experience – and not ancient authority – was of utmost importance to their knowledge.[27] The production of certain knowledge was thus closely interwoven with the embodied processes and actions of making, and especially in making things that imitated or replicated nature itself. Interest in the early modern craft practices that were central to this artisanal epistemology led Smith to adopt and develop the methods of scholars who have reconstructed historical experiments, recipes, and technologies as a means of doing history.[28] Smith instituted The Making and Knowing Project, part of which is directed toward the reconstruction of artisanal recipes in a middle-French manuscript book of secrets to investigate knowledge situated in practices of making.[29]

Smith has also drawn from anthropologists and philosophers of embodied cognition to try to bridge the gap between theory and practice. Others, such as the contributors to the volume *The Mindful Hand*, focus on intersections

[24] Long (2011). On the art/nature relationship in alchemy, see Newman (2004).
[25] Smith (2004). [26] Smith (2004, 95). [27] Smith (2004, 20, 6).
[28] Smith and Beentjes (2010). For a recent survey of historical reconstruction or reworking of experiments, see Fors, et al. (2016).
[29] This project, which I participated in as a post-doctoral researcher 2014–2017, will also produce a digital critical edition of BnF Ms. Fr. 640. For other aspects of the project, see www.makingandknowing.org.

between mental and manual labor and "question the efficacy of organizing historical inquiry around supposedly oppositional categories such as science/technology, scholar/craftsman and mind/hand."[30] These dyads, as the next section shows, have a long philosophical history.

Techne, *Episteme*, and the Manual Arts

Early modern European artisanal and craft knowledge evolved in a context that took into account ancient philosophy. At least from the time that philosophers in ancient Greece began providing written accounts of the study of wisdom, it was argued that there were different types of human knowledge and that these could be categorized in a hierarchy. Especially following Aristotle, philosophers generally viewed practical knowledge (*techne*) as lesser than theoretical or philosophical knowledge (*episteme*). *Techne* was thought of as the knowledge of how to make things or produce effects, and was mostly the domain of craftspeople. It was gained through sensation of the physical world rather than reason, and was thus regarded as too unreliable and indefinite for revealing fundamental truths – that is, philosophical knowledge. *Episteme* on the other hand was gained through deduction and logical reasoning – perhaps best exemplified in mathematics – and was regarded as the most certain and least contingent type of knowledge.

Manual labor and craft work were generally the domain of *techne* and thus could not lead to certain, philosophical knowledge. Such work then, while productive and not entirely divorced from knowledge, had a lower intellectual and social status, and was generally the domain of slaves, whereas free and leisured philosophers enjoyed a higher status. Aristotle argued that unlike philosophical reflection, a craftsperson's *techne* could not lead to virtue because it did not arise from a fixed disposition of character and was directed at the end of making something rather than pursued as an end in itself. In his *Politics* he put it bluntly: "citizens must not lead the life of artisans or tradesmen, for such a life is ignoble and inimical to excellence."[31]

This general hierarchy was also accepted by many Stoics, who viewed *techne* as less stable than *episteme* and argued, as Seneca put it, that all pursuits besides "the study of wisdom," even other intellectual pursuits, are "puny and puerile."[32] In *De finibus*, Cicero stated that those who do manual labor to earn a wage are like slaves, and he dismissed craftwork and manual labor as

[30] Roberts (2007, 3).
[31] Aristotle, *Politics* (1328b36–39) (Aristotle 1984, 2:2109).
[32] Seneca, Letter 88.

vulgar, servile, and illiberal.[33] Indeed, many Romans harbored equally negative views about practical arts, labeling them *artes illiberales* or *artes vulgares*, among other pejoratives.[34]

Along with the rise of Christianity came the view that the Fall had left the ground cursed and hence the labor of tillage an arduous punishment for sin.[35] Early Christian thinkers like Augustine of Hippo sometimes portrayed work as a distraction from divine pursuits, and in the Middle Ages, monastic movements regarded labor as a means of penance necessary for meeting the needs of the community. Benedictine monks, for instance, carried out diverse manual tasks to avoid idleness – "the enemy of the soul," as Benedict put it in his *Rule* – which could include gathering crops, writing in a scriptorium, and even the production of medicines.[36]

In the ninth century, John Scotus Eriugena used the term *artes mechanicae* in reference to seven unnamed manual arts that paralleled the *artes liberales*, but were differentiated by their earthly rather than divine orientation. In the twelfth century, Hugh of Saint Victor wrote an influential text titled *Didascalicon* that identified and described seven particular *artes mechanicae*: fabric making, armament, commerce, agriculture, hunting, medicine, and theatrics. Hugh understood these categories broadly, and for instance conceived armament to include architecture, carpentry, and metalworking. His extensive descriptions of the manual arts emphasized handicrafts instead of practical mathematics, and, importantly, he elevated the status of the *artes mechanicae* and classified them as one among four major divisions of philosophy.

Beginning in the thirteenth century, the elevation of manual arts was reinforced by an influx of new ideas from freshly translated Arabic texts that esteemed craft knowledge and viewed artisanal labor and the construction of instruments as compatible with philosophy. Some authors began to emphasize practical mathematics, the science of weights, and also the science of machines, which encompassed the knowledge of mechanical instruments, from lifting engines to optical devices to musical instruments. Roger Bacon argued similarly, but divided practical geometry into several parts, including, for instance, agriculture, building of weapons, and metrology. Bacon was also among the medieval authors influenced by Arabic alchemy, which combined the work of the head and the hand perhaps more than any other practice. Texts attributed to Jābir ibn Hayyān (c. 721–c. 815) and Al-Rāzī (854–925) were

[33] Summers (1987, 243–244). [34] Hansson (2016, 65). [35] Genesis 3:17.
[36] The Benedictine motto *"Ora et labora"* likely dates from the nineteenth century. See Meeus (1992).

rich with descriptions of alchemical manipulations, such as distillation and transmutation, but also contained theories about the nature of matter.[37] Nevertheless, while the reclassification of the manual arts and the emergence of alchemy had a notable effect on European intellectuals, neither had a substantial influence on the curricula of the medieval university, which remained focused on the liberal arts.

Artisans and Their Knowledge

At the end of the Middle Ages and the beginning of the Renaissance, the gulf between practical and theoretical knowledge began to narrow considerably. In the fourteenth and fifteenth centuries, certain artisans and scholars began to codify knowledge of practical subjects in technological treatises. Konrad Kyeser was a German military engineer originally trained as a physician who authored *Bellifortis* (c. 1405), a military treatise in Latin verse based on experience he had gained in battles against the Ottoman Turks.[38] The illustrated text includes discussions and depictions of siege engines, rockets, firearms, instruments of torture, and a large variety of other military technologies.[39] At around the same time, Michalli da Ruodo, or Michael of Rhodes, sailed on more than forty voyages aboard commercial galleys or war galleys of the Venetian navy, eventually working his way up from common oarsman to *armiraio* – a high position that served as captain and navigator of a convoy of ships.[40] Despite a lack of formal training in letters, Michael wrote two remarkable manuscripts that include a 90-folio treatise on commercial mathematics, a treatise on shipbuilding, and an autobiographical account of his many voyages.

On the heels of this manuscript tradition, so-called "books of secrets" containing descriptions of practical and useful techniques related to craft knowledge were printed, beginning in the final decades of the fifteenth century and proliferating through the seventeenth century.[41] Other popular how-to manuals and texts that circulated widely focused on husbandry, culinary or medical recipes, and household oeconomy.[42]

While such practical treatises were widely read, it was not until the late Renaissance that there emerged a prominent appreciation among philosophers for artisanal work and its ability to produce knowledge useful for

[37] The name of the possibly legendary ibn Hayyān was Latinized as Geber; al-Rāzī became Rhazes.
[38] Whitney (2004, 112). [39] Long (2001, 105–110). [40] Long (2009). [41] Eamon (1994).
[42] See Tribe (2005); Leong (2019).

understanding nature. The new status of artisans resulted partially from larger cultural changes related to commerce and the increasing demand – especially in Italian city-states – for beautiful, worldly goods and luxury objects. Matteo Valleriani has described this situation in early modern Europe as a "peak of density of activity," denoting a "constant increase in the intensity of practical activities" that led to "increased codification of knowledge."[43]

Some of the clearest examples of this epistemic transformation and the elevation of the intellectual and social status of practitioners come from Renaissance humanists and artists. The influential fifteenth-century German scholar Nicholas of Cusa wrote several works praising unlettered *idiotae* who gained knowledge of God's creation from things themselves and not merely from traditional learning.[44] The paragon of humanism Leon Battista Alberti was so impressed by Filippo Brunelleschi's work on perspective that he dedicated his book *Della pittura* (1435) to the artist and architect, even addressing him directly in the preface and requesting his expert feedback. Alberti wrote:

> Please, therefore, read my work carefully, and if anything seems to you to need amendment, correct it. No writer was ever so well informed that learned friends were of no use to him: and I want above all to be corrected by you, so as not to be criticized by detractors.[45]

Beyond feeding the cult of Renaissance celebrity and helping to give rise to the concept of the artist as genius, Alberti's text also sought to elevate the status of painting to the level of a liberal art by combining it with geometry. Alberti likewise wrote about other topics in the arts such as sculpture and architecture, and he even practiced painting and architecture in addition to his other philological and literary pursuits.

By the mid-sixteenth century, experience and practice had risen in esteem, and authors increasingly sought to remedy Scholastic Aristotelianism's disregard for practical knowledge. Mining and metallurgy provided fertile soil for such redresses, which were produced by artisans as well as scholars. For instance, the mining engineer Vannoccio Biringuccio wrote *De la pirotechnia* (1540), a practical text written in Italian that was effectively the first account of sixteenth-century mining techniques and explained such processes as the extraction of metals, cannon-making, and gunpowder manufacture. Shortly thereafter, the humanist scholar and university professor Georg Agricola

[43] Valleriani (2017, 4). [44] Oosterhoff (2014). [45] Alberti (1991, 35).

published *De re metallica* (1556), an extensive and comprehensive work on mining and metallurgy written in Latin for a learned audience.

Whereas Biringuccio humbly suggested the techniques he described could help meet human needs and Agricola modestly proposed that his descriptions of practitioners' knowledge could benefit other arts and sciences, Theophrastus Paracelsus von Hohenheim sought revolution. Otherwise known simply as Paracelsus, the itinerant physician became one of the most bellicose sixteenth-century voices against the Scholastic natural philosophy and Galenic medicine taught at universities, and practical knowledge was central to his assault. As he wrote in *On the Miners' Sickness and Other Miners' Diseases* (1534),

> For who could be taught the knowledge of experience from paper?, since paper has the property to produce lazy and sleepy people, who are haughty and learn to persuade themselves and to fly without wings. . . . Therefore the most fundamental thing is to hasten to experience.[46]

Paracelsus praised the work of craftspeople, farmers, and others who labored with their hands; he lauded the experience of travel and called for reading of the "*Codex naturae*" (i.e., the book of nature) from "land to land"; and he drew from the observations, recipes, and experiences of practitioners, especially miners, to support his new vision.

Paracelsus also drew from medieval medicine and alchemy to support this vision. He founded his new philosophy upon the practice of *Scheidung*, or analysis – generally, a distillation by fire – of a given natural substance into its constituent parts. Based on these distillations, he concluded that everything in nature was composed not merely from the ancient alchemical pairing of sulfur and mercury, but also salt, referring to these together as the *tria prima*, or three primaries. Paracelsus conceived of the analysis of compound bodies not merely as a separation of the constituents of matter, but also as a purification from toxic matter, likewise suggesting that the recombination of separated essences could yield exalted variants of the original substances, "cured" of their material dross. He called this whole process of separation and recombination *spagyria*, and Paracelsus's confidence in this alchemico-medical system led him to declaim against the traditional authorities:

> Let me tell you this: every little hair on my neck knows more than all your scribes, and my shoe-buckles are more learned than your Galen and Avicenna, and my beard has more experience than all your high colleges.[47]

[46] Paracelsus (1941, 91). [47] Paracelsus (1988, 6).

Paracelsus's emphasis on practice and his iconoclasm became a powerful force for supplanting traditional conceptions of nature and experience in the late sixteenth and seventeenth centuries.

Others in the sixteenth century developed similar philosophies that explicitly elevated the value of practitioners' experience over scholars' ruminations. In his treatise *Dell'oreficeria* (On goldsmithing, 1568), the Italian goldsmith Benvenuto Cellini wrote:

> I consider that practice always comes before theory in every discipline, and that rules of theory, in which your skillful craftsman is accomplished, are always grafted onto practice afterwards.[48]

Similarly, Bernard Palissy, the French Huguenot craftsman famous for his ceramics, directly privileged practice over theory and supported an empirical philosophy rooted in craft knowledge. In the preface to his 1580 *Discours admirables de la nature des eaux et fontaines*, Palissy argued in favor of what he called "the philosophy of laborers." In the dialogue itself, he pitted "Practice" and "Theory" against one another. Practice always emerges victorious, proclaiming, "I have had no other book than the sky and the earth, which is known to all, and it is given to all to know and to read in this beautiful book."[49] For Palissy, practice gives birth to theory, and not vice versa, whether considering ceramics, alchemy, military matters, or navigation. Contrary to bare theory divorced from experience, practice has philosophy built into itself and is the only means of giving rise to reliable theory. In this way, Palissy attempted a kind of reconciliation between practice and theory, but this was really a reconceptualization and demotion of theory.

These authors argued – sometimes acerbically – for the elevation of the intellectual and social status of practice and practitioners. Yet, such polemics should not lead to the conclusion that this was the only paradigm for relations between practitioners' knowledge and learned, philosophical knowledge in early modern Europe. Instead, as Long has argued, there were significant interchanges in trading zones – especially arsenals, mines, and sites of building projects – where people from diverse backgrounds with different views and assumptions met and exchanged substantive information.

Renaissance building projects were especially conducive for exchange between artisans, architects, engineers, humanists, and mathematicians. Many deftly moved between the world of humanist erudition and practical know-how, demonstrating that, in addition to information being exchanged

[48] Quoted in Long (2001, 239). [49] Quoted in Smith (2004, 158).

between discrete social groups, boundaries between these groups were shifting. Whereas in the Middle Ages, building projects generally were supervised by master masons or master carpenters, in the fifteenth century, these tasks increasingly were given to artists who were distinguished from master builders.[50] At the same time, the building profession was not well demarcated and lacked the guild laws and apprenticeship system used by other artists and craftsmen. The figure of the "architect" emerged and expanded in the Renaissance amidst a number of specific cultural and practical changes.

Architects drew extensively from the recently rediscovered treatise *De architectura* by Vitruvius (c. 85 BC – c. 15 BC), who had noted the significance of artisans' knowledge to architectural projects as well as the general significance of both theory and practice. The importance of this knowledge was reemphasized in Vitruvian texts and commentaries, such as a 1556 edition by Daniele Barbaro, a wealthy humanist scholar. Barbaro collaborated closely with the architect Andrea Palladio, who began his career apprenticed to a stonemason and eventually worked among diverse builders, artists, and artisans before becoming enmeshed in a circle of wealthy humanists and patrons in the Veneto. Palladio contributed his close understanding of building trades and materials to Barbaro's text and also wrote texts of his own.[51]

In addition to the humanist rediscovery of Vitruvian architecture, new drawing methods employing the science of perspective and new geometrical and mathematical techniques were developed. Likewise, new methods for visual representation in printed books allowed for more precise and lavish descriptions of plans for architectural projects. Finally, and perhaps most significant, a state of nearly incessant warfare in Europe called for the rapid expansion of military architecture, including new methods for fortification that employed mathematics and precise surveying practices. In effect, over the course of the long and violent sixteenth century, the architect's purview expanded considerably, and to meet these new demands, his fields of expertise grew to encompass additional practical and erudite knowledge.

Francesco di Giorgio was a notable architect and engineer in the humanist tradition, and his written work on architecture is emblematic of many of these developments. He originally trained as a painter in a Sienese workshop before working as a military engineer and executing diverse designs for fortifications, hydraulic systems, and sculptural projects. He wrote an

[50] Lefèvre (2017). [51] Long (2011, 120–123).

architectural treatise entitled *Trattato di architettura, ingegneria e arte militare* that displayed a deep technical and mathematical knowledge, as well as a humanist engagement with literature and the Vitruvian tradition.[52] The first part of the *Trattato* demonstrated Francesco's work as a practicing engineer and contained numerous drawings and technical descriptions of machines, from mills to more sophisticated military engines, while the second part contained a greater degree of theoretical discussion and humanist erudition.

Architects, artist-engineers, and other "Vitruvian artisans" came to embody the Renaissance ideal of the "polymath," and central to this identity was dexterity in diverse crafts, as well as both practical and theoretical mathematics. Among his extraordinary investigations and artistic output, Leonardo da Vinci praised geometry and argued that "mathematical demonstration" and the "test of experience" were central to science, which he defined as "that mental analysis which has its origin in ultimate principles beyond which nothing in nature can be found which is part of that science."[53]

Shortly after Da Vinci's heyday, the Cremonese artisan Janello Torriani made a name for himself as Holy Roman Emperor Charles V's clockmaker, an inventor of mechanical devices and automata, and an author of mathematical treatises for Philip II of Spain (among many other endeavors). Contemporaries praised Torriani's "ingenuity," especially in regard to his construction of the first spring-driven movable planetary clock, noting that he constructed it himself "at the anvil with his own hands" and carried out all of the mathematical calculations for the orbits of the planetary bodies.[54] Torriani's exploits were especially celebrated because of his lowly origins and his lack of formal university training; but far from arising completely separate from university culture, he was tutored in mathematics and astrology by his close friend Giorgio Fondulo, a university-trained physician and philosopher. The historian Cristiano Zanetti has argued, *contra* Zilsel, that Torriani signifies "a less sharply separated representation of Renaissance artisanal and university knowledge," as well as a "less Manichaean contraposition between humanists and Aristotelians."[55]

Many of these architects and engineers represent how the social standing of mathematical practitioners dramatically changed during the early modern era. Mario Biagioli has shown that, in Italy, mathematics was initially marginalized on account of its association with the mechanical arts and its

[52] The *Trattato* only circulated in manuscript form. See Long (2011, 42–47).
[53] See Keele (1970). [54] Vida (1550), quoted in Zanetti (2016, 113). [55] Zanetti (2017).

disregard by the Aristotelian philosophy of the late-medieval university, but that its standing was much improved by 1600.[56] Among the diverse causes for these changes were the rise of absolutist states, the corresponding revolution in warfare and fortification methods, and the development of a new court and university culture. In short, practical and applied mathematics became a valuable component of civil and military affairs.[57] Indeed, Pascal Brioist has concluded that the situation in Europe had changed so much by the latter half of the sixteenth century that the appearance of having only impractical scholarly knowledge could harm a mathematician's credibility; instead, a combined practical and theoretical knowledge was integral to those serving the state and at court.[58]

Niccolo Tartaglia's *La nova scientia* (1537) is an archetype of this combination of practical and theoretical discourses in the context of warfare. The "new science" Tartaglia's title proclaims is the mathematization of the art of gunnery and the application of geometry, algebra, and Archimedean methods of reasoning to ballistics. The study of projectile motion had traditionally been a part of natural philosophy and was discussed in largely theoretical terms. The turn to practical mathematics ultimately contributed to the prioritization of the authority of experience and was influential on later experimental and geometrical studies of motion, namely by Galileo in his similarly titled *Discorsi e dimostrazioni matematiche intorno à due nuove scienze* (1638).

Developments in the relationship between practical and erudite knowledge were certainly not limited to Italy; throughout Europe, the lines between different professions, traditions, and practices were becoming especially blurry during the sixteenth and seventeenth centuries. In one salient example, Vera Keller has argued that the Dutch engineer, alchemist, and philosopher Cornelis Drebbel is emblematic of complex entanglements between artisanality and philosophy.[59] Drebbel's inventions included a submarine, diverse optical devices, a scarlet chemical dye, and an ostensible perpetual motion machine. His fusion of natural philosophy, alchemy, engineering, and craft knowledge in the diverse and ambitious projects he undertook earned him considerable fame during his lifetime, and, as Keller notes, far from damaging his status, diverse authorities championed him as an "artisanal philosopher."[60]

[56] Biagioli (1989). [57] See also Johnston (1994). [58] Brioist (2009). [59] Keller (2013).
[60] Keller (2008).

Practitioners and the New Philosophy

Over the sixteenth and seventeenth centuries, the practical and useful knowledge that had been the domain of artisans became a hallmark of the new science, and this was nowhere more evident than in the works of the English natural philosopher and statesman Francis Bacon. Bacon ridiculed the natural philosophy of his day as powerless and useless, arguing that it ought instead to be replaced with an "active science" focused on practical ends and used for the good of the state and humanity in general. The mechanical arts and their productive works were exemplars of Bacon's "Instauration" or vision of the reform of natural philosophy. He pointed especially at the printing press, gunpowder, and the compass as groundbreaking technological developments that had radically transformed the world. Inspired by such inventions, Bacon called for a "total reconstruction" of all human knowledge grounded on a methodology that prioritized observations and experiments. Moreover, this new, active science would not be practiced through solitary contemplation, but would instead require a community with institutional support.

Bacon argued against the traditional condescension toward the mechanical arts, showering special praise on their productive works, but when discussing artisans and craftsmen he was often skeptical about the ability of such "empirics" to produce reliable knowledge. In Bacon's vision for the new science, it was natural philosophers who played the most important role in generating knowledge. Even so, recent scholarship has demonstrated that Bacon's relationship with artisans and entrepreneurs was more complicated than his pronouncements might suggest. Cesare Pastorino has shown that Bacon's philosophical discourse regarding experimentation drew heavily from Bacon's own concrete knowledge of the works of early Stuart craftsmen.[61] In his capacity as solicitor and attorney general, Bacon was familiar with a variety of royal privileges and patents for novel technical inventions, and he even supervised the drafting of many of these formal requests. Pastorino shows that such legal work had an important influence on Bacon's philosophical texts, wherein he adapted legal requirements used to identify new inventions to delineate and scrutinize the practices of experimentation.

As the new science was institutionalized and the members of the Royal Society of London attempted to implement Bacon's vision, they maintained an interest in the information that artisans and empirics could furnish, but generally distinguished their gentlemanly philosophical enterprise from such

[61] Pastorino (2017).

lowly affairs. Bishop Thomas Sprat, a founding member of the Society, asserted that although artisanal knowledge was useful to natural philosophy, artisans themselves were not fit to join the Royal Society because their profit-minded motives and general busyness did not befit leisured philosophical reflection.[62] For Sprat, the empirical practices of artisans and craftsmen were ultimately to be appropriated rather than emulated.[63] That these distinctions between classes were important in the Royal Society is illustrated in a manuscript by John Flamsteed, where the astronomer referred to Robert Hooke as an impudent "Mechanick Artist," likely on account of Hooke's close familiarity with mechanical devices in his capacity as the Royal Society's curator of experiments. For his part, Hooke had a more robust view of the interconnectedness of artisanal work with the new science as well as the status of artisans. In the preface to his *Micrographia* (1665), he wrote, "all my ambition is, that I may serve to the great Philosophers of this Age, as the makers and grinders of my Glasses did to me."[64]

Conclusion

Across the discipline of the history of science, there are and will likely continue to be divergent opinions on the relative importance of practitioners' knowledge in the emergence of science during the early modern era. Recently, general historiographical trends have amplified broader narratives that feature artisanal and practical knowledge. At the same time, traditional narratives in the history of philosophy and the history of science regarding the uniqueness and discreteness of the Scientific Revolution have garnered less traction among historians. Over the past several decades, the move toward a more global perspective combined with an increasing appreciation for postcolonial critiques has led many to question the classical, Eurocentric narrative of the rise of science. Indeed, such shifts combined with recent work on the importance of practitioners' knowledge have led some historians to propose that the history of science should redefine itself as the history of knowledge.[65] Whether or not this transformation is successful, it appears that historians of early modern science will have to grapple with a broader understanding of the history of knowledge, one that includes artisans and practitioners.

[62] Sprat (1667). [63] Young (2017). [64] Hooke (1665).
[65] Daston (2017). In addition to the 2017 establishment of the journal *Know (A Journal on the Formation of Knowledge)*, *The Journal for the History of Knowledge* was launched in 2020.

11

Medicine and the Science of the Living Body

PETER DISTELZWEIG AND EVAN RAGLAND

Introduction

The learned medicine of the mid-sixteenth century was a venerable, sophisticated, culturally prominent intellectual tradition with robust institutions. Especially in the first decades of the century, humanist efforts to recover, edit, translate, and master a range of ancient medical texts, especially works by Galen, had greatly enriched the medieval and early-Renaissance medicine cultivated in the university medical schools.[1] Learned medicine could offer extensive theoretical and descriptive resources by which to understand the living body, health, and disease, as well as to rationalize practical interventions. It also encompassed explicit methodological reflections and a variety of specific practices of inquiry. In addition, this tradition was systematically, even by definition, devoted to the application of theoretical expertise to practical (often life-and-death) contexts. For all these reasons, it is no surprise that a large percentage of members of the early modern scientific societies were physicians, nor that many early modern figures associated with theoretical and methodological novelties explicitly engaged with medical thought and were engaged, in turn, by the heirs of that tradition.

In this chapter, we outline important theoretical and methodological facets of elite medicine of the sixteenth and seventeenth centuries and trace some developments in each, highlighting their significance for the history and philosophy of the Scientific Revolution. We look first at "theoretical" questions (centered on medical *physiologia*) and then at "methodological" ones (focused on the role of observation, experience, and experiment). Much of

[1] Siraisi (2007) provides a useful introduction to late medieval and early Renaissance medicine. See Bylebyl (1979b) on the humanist Galenic revival.

the history of medicine in our period can be helpfully understood as a web of interactions between these theoretical and methodological developments.

Medical *Physiologia*

In the preface to *Medicina* (first published in 1554), the physician and medical theorist Jean Fernel identifies *physiologia* as the first of the five parts of medicine, together with pathology, prognosis, hygiene, and therapeutics.[2] Pathology "investigates the diseases and affections deviating from nature that can threaten human beings, their causes and symptoms." Prognosis "explicates the symptoms by which the doctor can know what is about to happen, who will run the course of a disease and who will die." Together with *physiologia*, these three constitute the "theoretical" part of medicine, because, Fernel says, they terminate in simple cognition of things. The remaining two parts belong to the "practical" part of medicine, because they terminate in action. Hygiene "by good rules for living preserves the good constitutions of the body, wards off threatening harm, and at the same time discerns the proper pattern of life adapted to the needs of the sick." Therapeutics, in turn,

> wards off sickly affections of the body, returning it to health, by the use and application of medical resources. As the culmination of all medicine, it methodically stores up technique and furnishes various defenses by which to aid effectively both the whole body and any of its parts that suffer.[3]

If therapeutics is the culmination, *physiologia* is the foundation of medicine, both theoretical and practical, since it "concerns itself with the nature of the fully healthy human being and all its powers and functions."[4] This "natural part of medicine" overlaps with natural philosophy (which was also frequently called *physiologia*). Fernel devoted *Medicina*'s first part to *physiologia*, and a sketch of its structure and content provides a helpful glimpse of the mid-sixteenth-century state-of-the-art and gives useful context in which to understand two revisionist developments: "chymical" approaches, especially influenced by Paracelsus; and mechanistic ones, inspired particularly by Descartes.

Fernel's *Physiologia* is broken into seven books of unequal length. By far the longest is the first, which provides a lengthy and detailed "Description of the Parts of the Body." The presentation is organized primarily around functional

[2] For a brief orientation to Fernel see Henry and Forrester's introduction in Forrester's translation of the *Physiologia* (Fernel 2003).
[3] Fernel (1555, 13–14). [4] Fernel (1555, 13–14).

categories, starting with the skeleton, connective tissue, and muscles, before discussing the internal organs according to the "three bellies" – the abdomen (containing parts devoted to nutrition and generation), the thorax (containing the heart and lungs, devoted to "vital" functions), and the head (containing the brain, "the citadel and dwelling of the human mind, the abode of thoughts and of reason, the wellspring of movement and of every sense"[5]). Then nerves, veins, and arteries are described, together with membranes, skin, flesh, marrow, fat, hair, and nails. Fernel states that the doctor's mastery of this detailed account is derived from human dissection, directions for which he provides at the end of the book.

Fernel explains the need for *physiologia* to start with this sensate knowledge in Neoplatonic terms: the human mind, thrust into the prison of the body, must begin with the sensible and obvious. Sound knowledge of the human body, derived from frequent dissection, provides the foundation and starting points for all medical expertise, as well as a geographical knowledge of the arena of health and disease. Nonetheless, insists Fernel, the doctor must not think this sensate knowledge of the human body, even supplemented by undergraduate dabbling in philosophy, suffices for understanding the nature of the healthy human. Before turning to the rest of medicine, the doctor must penetrate beyond the visible, using reason to reach hidden principles and causes. Only then will the doctor have complete knowledge of the nature of the healthy human being.

In the remaining six books of the *Physiologia*, Fernel takes up this task, providing his own synthesis of broadly Aristotelian and Galenic approaches (with a strong Neoplatonic element). In turn, he treats the material composition of living things out of the four Aristotelian elements (earth, air, fire, and water); the nature of their mixtures and the status of the elements in mixed bodies; and the nature of temperament (the *ratio* of the mixture of elements), so important for human health. Besides these corporeal principles common to all natural bodies, Fernel identifies in living things a special innate heat, three special humors (fatty, alimentary, and watery),[6] several kinds of spirits (innate, natural, vital, and animal), and "original moisture." While still corporeal, these principles are present only in living things and are intimately connected to faculties derived from the soul and not merely from the material composition of the body. Fernel surveys these faculties, categorized most broadly as natural, animal, rational, and vital. They account for

[5] Fernel (2003, 101).
[6] These more fundamental humors are in addition to the more familiar Galenic humors.

physiological activity inexplicable in merely material-structural terms and include, e.g., formative or plastic powers involved in fetal development and selective attraction involved in many physiological processes. Fernel then provides an account of physiological functioning, describing the actions of various organs and organ systems and explaining the generation and behavior of the four Galenic humors (blood, phlegm, yellow bile, and black bile). Finally, the *Physiologia* concludes with a treatment of human procreation, characterized as an inquiry into the ultimate causes of everything so far discussed. The development of the complex living human is traced to its ontologically simple principles and causes, including immaterial, artistic, and end-oriented ones.

Fernel's systematic (and sometimes idiosyncratic) work constitutes a synthesis of a wide range of topics and controversies drawing from primarily Aristotelian and Galenic resources, marked at various points by an insistence that it is ordered to the needs of medicine. The account of the human being provided depends on detailed anatomical description; analysis of the body into complex, hierarchical functional systems; a rich set of matter-theoretical resources; and, finally, the soul, a distinct principle upon which ultimately depends a wide range of both organ-specific and general capacities in the living human body. Though incorporating novelties drawn, for example, from Neoplatonists and the atomist Lucretius and meant to be cutting-edge, Fernel's *Physiologia* was not revolutionary in aspiration. It provides perspective from which to appreciate more revolutionary developments in medical *physiologia*.

Alchemical, Chymical, Paracelsian Bodies

Alchemy influenced medicine before the early modern period, notably in John of Rupescissa's fourteenth-century elevation of the spirit of wine (largely distilled ethanol) to a central place in his theological medicine as the "water of life" and "quintessence" that resisted corruption and death. Yet in the sixteenth and seventeenth centuries, "chymical" approaches to the living body became more widespread, diverse, and self-consciously revisionist, especially in the wake of Paracelsus.[7]

Around 1529, Theophrastus Philippus Areolus Bombastus von Hohenheim took on the name 'Paracelsus', likely to signal that he had surpassed the

[7] We use 'chymistry' and related terms to refer to the new diversity and elevation of alchemical *ars* in the sixteenth and seventeenth centuries, resulting from the transformations of medieval "alchemy," and following Newman and Principe (1998).

ancient authorities, represented by Celsus, the ancient Roman encyclopedist whose medical compendium remained an important source. Against the Aristotelian view of logical demonstration as the ideal form of *scientia*, Paracelsus equated knowledge with experience (*experientia*, *Erfahrung*).[8] More precisely, he held that God placed *scientia* within natural things, which teaches each to realize its own perfection, and that "*Experientia* is knowledge of cases in which *Scientia* has been put to the test."[9] Natural knowledge thus cannot come from books, since the investigator must identify and pursue unity with the thing to be known. For Paracelsus, God created the world through processes of separation (*Scheidung*), which continue in the material generation and destruction of things. Work in the chymical laboratory harnesses these processes and separates material dregs from true natural principles imperceptible in Galenic diagnosis by superficial examination of urine or the pulse. Analysis by fire produces a "vital anatomy," unlike the dead anatomy of dissected cadavers stressed by learned physicians like Fernel.

Paracelsus's complex ontology emphasized spiritual non- and quasi-material entities, in conjunction with sense-guided technical practice. Like his Neoplatonic predecessors, such as Marsilio Ficino and Agrippa von Nettesheim, Paracelsus emphasized the causal power of the "invisible" world over the material world, and he denied the importance and even the reality of humoral mixture. Instead, he sought to reveal spiritual *semina*, astral bodies, and images, which acted in specific, visible laboratory and bodily processes. These spiritual or astral entities partake of both the formal, ideal world and the material world of bodies. In key treatises, Paracelsus traces minerals, metals, plants, human bodies, diseases, etc., to *semina* acting on matter, rather than to mixtures of the Aristotelian elements or Galenic humors. In some of his influential works, Paracelsus also advocated for a new theory of chymical principles, adding salt to the medieval alchemists' sulphur and mercury. Pointing to the Christian Trinity, Paracelsus argued that God's creation came through these three chymical principles. These original principles were not isolatable in the laboratory, and many different salts, sulfurs, and mercuries gave objects properties of solidity, flammability, and volatility, respectively.

In contrast to Galen's humors and faculties, which he styled as anti-Christian materialism taught by a pagan, Paracelsus described the living body's selective and creative processes as chymical operations fashioned by indwelling spiritual craftsmen, or *archei*, which govern everything that happens in a living body. When impaired or enraged by the invasion of foreign

[8] Pagel (1982b, 58). [9] Paracelsus, *Labyrinthus medicorum errantium*, in Pagel (1982b, 60).

semina, spirits, images, or *archei*, these *archei* do not properly direct the material bodily processes, and chymical deposits ("tartar") accumulate in the body. Diseases in this view are not passing "deviations from nature" that impair bodily function, as in Fernel's Galenic definition, but real, distinct entities (*entia morbis*). Thus, some scholars have credited Paracelsian medicine with an "ontological conception of disease."[10]

Another physician, Girolamo Fracastoro, published a rather different ontological theory of disease and contagion in 1546, couched in language and sources familiar to physicians in the Hippocratic and Galenic traditions.[11] Galen occasionally had discussed "seeds of disease" in remarks on contagious transmission, infection from the air, and the regular recurrence of diseases. But his quest for medical certainty and emphasis on disease as the impairment of an individual's idiosyncratic healthy functioning militated against expanding the concept of diseases to include a host of separate entities. Fracastoro's primary inspiration was Lucretius's atomistic theory of the seeds of things. He divided contagion into three types – by contact, by *fomites* (material deposits), and by action at a distance – and argued each really came about from the invisible, material "seedlets of contagion" (*seminaria contagionis*) spread by exhalations, the air, clothes, etc. Fracastoro's readers took up his language and threefold division of contagion, while associating his *seminaria* with earlier means of contagion such as miasmas, poisonous exhalations, pestiferous odors, etc.[12] In this way, they domesticated his innovative ideas, using them widely but without revolutionary ambitions.

Paracelsian and other forms of chymical philosophy and practice heavily influenced ways of understanding the living body into the seventeenth century. Jan Baptist Van Helmont produced the most influential chymical medicine of the seventeenth century, attracting self-identified followers in France, Germany, Italy, the Low Countries, and especially England. Van Helmont took up many of the main themes of Paracelsus: the rejections of demonstrative logic, Galenic humoralism, and the Aristotelian elements; an emphasis on the agency of spiritual and quasi-material entities over material causation; the search for a Christian philosophy and medicine; the importance of chymical analysis; and even specific terms, such as *archeus*. In his own view, Van Helmont's theory of disease rejected rude materialism for spiritual, Christian reality. Spiritual *semina* are enmattered in corporeal "husks" throughout nature. Each combines the genuine causal agent, the *archeus* – a kind-specific, subtle vapor that works internally according to its intelligence – and

[10] Pagel (1982a, ch. 5). [11] Nutton (1983). [12] Nutton (1990).

the ferment, which is the "luminous" means by which matter is shaped into an external image and transmutations are accomplished.[13] Rabies, for example, is not generated in the bite of a rabid dog by humoral or corpuscular actions of the dog's saliva, but primarily by the ferments and the sympathetic interaction of the *archei* of the dog and human.[14]

Many subsequent physicians combined aspects of Galenic medicine with chymical analysis, theory, and remedies. Daniel Sennert was an influential reformer in this vein. Already by his 1611 textbook, *Institutiones medicinae*, Sennert applied atomistic, chymical explanations to physiology and pathology, an approach he expanded in many later works. Taking a more Aristotelian line, he rejected the semi-material *semina* of the Paracelsians, and instead conceived of the three chymical principles salt, sulfur, and mercury as the most fundamental *mixed* bodies composed of Aristotelian elements; these three principles in turn enter into further mixtures. Since Sennert's atoms are each a hylomorphic union of form and matter, complex things composed of atoms – especially living bodies – possess a hierarchy of substantial forms.[15] On this view, shared with his important source J. C. Scaliger, a living body possesses a single dominant substantial form, over a plurality of substantial forms, including the forms for all the minimal parts – for Sennert, the chymical atoms. Drawing from chymistry, as well as medical discussions of spontaneous generation, Sennert applied this theory of a hierarchy of substantial forms to a range of topics such as chymical reactions, poisons, diseases, medicines, and animal generation.[16] Importantly, Sennert began a chymical reform of medicine, and developed a detailed theory of elective affinity (*cognatio naturae*) to explain fermentation, the solution of metals in acids, disease states, and drug actions.[17] Later, Gottfried Wilhelm Leibniz, like Sennert, conceived of the living body as consisting of a plurality of forms, in hierarchical order, marked by notable chymical properties and processes.[18]

Descartes's Physiological Project in Medical Context

While clearly related to diverse threads in medical thought, the rise of mechanistic approaches to medicine in the seventeenth century can be traced

[13] Van Helmont (1652, 84); Partington (1961, 2:229–232).
[14] Van Helmont (1652); Pagel (1982a, 147).
[15] Newman (2006); Blank (2010). This is a variation on a standing alternative within Aristotelianism to the Thomistic insistence on a single substantial form.
[16] Arthur (2006); Hirai (2011). [17] Newman (2012); Klein (2016). [18] Smith (2011).

especially to René Descartes's rather different revisionist approach to the human body. In recent years, scholars have emphasized both the significance of Descartes to medicine and of medicine to Descartes.[19] For the latter, ample evidence is provided by Descartes's explicit articulation of medical preoccupations and by the attention he gave to medical topics throughout his career.[20] Perhaps the most important evidence of Descartes's importance to medicine is the level of explicit engagement with Descartes's *physiologia* by learned physicians and anatomists.[21] Some criticized and some championed its mechanistic aspirations – many early Cartesians were physicians – as well as defending or attacking its particular tenets.

Prominent in Descartes's writings is a project to provide a *physiologia* that accords with his broader natural philosophy – what Robert Boyle would later identify as a paradigmatic instance of the "mechanical philosophy." However, Descartes's physiological project is conservative in important ways. He takes for granted the basic adequacy, at the visible level, of received anatomical descriptions and functional localization and decomposition.[22] Thus, in article seven of *Passions of the Soul* (1649), Descartes sketches a standard function localization in the three Galenic organ systems: a nutritive system constituted by the stomach, bowels, liver, and veins; an animal system constituted by the brain, nerves, and muscles; and the vital system constituted by the heart, lungs, and vessels. Throughout, he emphasizes the lack of novelty of this sketch.[23]

However, in subsequent articles, when he provides a further analysis of *how* the various organs and parts perform their functions (e.g., how muscles contract and how that contraction depends on nerves), Descartes claims to and does in fact depart from standard accounts. He posits new, often (but not always) subvisible, structures that feature in primarily pneumatic or hydrodynamic accounts of living function. In the *Passions of the Soul* and other physiological texts, Aristotelian and Galenic elements, elemental qualities,

[19] Descartes (1972); Carter (1983); Grene (1993); Des Chene (2001); Fuchs (2001); Aucante (2006); Manning (2013).

[20] Aucante (2006) nicely collects these.

[21] Descartes's significance to the medical community reaches back at least to the early 1630s. We have documentation that suggests the University of Bologna sought to offer Descartes a chair in theoretical medicine in 1633 (Manning 2014).

[22] Important exceptions include his early adoption of Harvey's views on the circulation of the blood and of the heart as the source of that circulation – though he rejects Harvey's understanding of how the heart produces it – and Kepler's treatment of the eye.

[23] Only when he speaks of the heart, lungs, and the circulation of the blood is there a note of novelty; but Descartes credits Harvey and insists all embrace the circulation who examine the matter with "open eyes" (Descartes 1989, 21–22).

and temperaments are replaced by matter as mere extension in motion. Although "animal spirits" are invoked, they are described in terms of his mechanical matter theory and again act primarily according to pneumatic or hydrodynamic principles. They have no special connection to soul nor special material composition. Nor are they carriers of faculties derived from soul and unique to living things. The basic functioning of the living body (nutrition, respiration, the heartbeat and blood circulation, muscle contraction, etc.[24]) is explained in terms of principles common to non-living things. Thus, he emphasizes that (unlike Galenic "innate heat") the heat in the heart is the same in kind as that in fermenting hay. Descartes thus replaces the Galeno-Aristotelian matter theory with a mechanical one and eliminates principles like vegetative and animal faculties and souls.[25]

Descartes's rejection of end-oriented physiological principles, along with his refusal to appeal to divine purposes, introduces an important problematic into his thought: How does one identify the *healthy* human being and ground its *normative* status? Relatedly, is Descartes's extensive *functional* characterization of the human body ultimately ungrounded? Descartes's discussions of the mind–body union, the import of the senses, the meanings of "nature," and the possibility of a "true error of nature" reflect this problematic.[26] Though there is scholarly debate on important aspects of this topic, Descartes's physiological project seems to consist primarily of a kind of functional analysis or mechanistic explanation of empirically discovered, recognizably similar, stable complex systems. Such functional explanations need not invoke natural normativity nor ontologically simple, end-ordered natural principles. Still, many later seventeenth-century Cartesians would find Descartes's explanatory constraints untenable in the face of the functional complexity of human and other living bodies.[27]

Descartes's mechanistic program combined with related strains of thought in Galileo Galilei and others to shape later physiological research. "Iatromechanists" continued to look for more micromechanical explanations for bodily actions. Later mechanist anatomy and medicine tended to focus

[24] Indeed, as Hatfield (2007) emphasizes, Descartes applies the same strategies to the brain in order to "mechanize" much (though of course not all) of what we might call psychology.

[25] Hall's introduction and notes in his translation of *Treatise on Man* provide a thorough examination of the relations between Descartes's physiology and early modern medical physiology (Descartes 1972).

[26] MacKenzie (1975); Rodis-Lewis (1978); Des Chene (2000b); Gaukroger (2000); Des Chene (2001); Simmons (2001); Shapiro (2003); Brown (2011); Manning (2013); Detlefsen (2013); Distelzweig (2015).

[27] Des Chene (2005).

more on the solid parts of the body and their structures than on the humors in understanding bodily action, disease, and health. For example, Robert Hooke rejected Descartes's speculative micromechanisms and used the microscope to "peep in at the windows" of nature's usual course, without "dissecting and mangling creatures whil'st there is life yet within them."[28] He found that the sting of the nettle came from the prick of tiny syringes, and flies clung to glass with tiny hooks. For many, such as leading anatomist and physician Marcello Malpighi, the mechanistic approach flowed from a commitment to the uniformity of nature: changes in living things occurred through the same processes found elsewhere. Malpighi defended the usefulness of mechanical philosophy for medicine, noting the changing bases of explanation from faculties and qualities to structures and particles. Excessive urination, rather than being caused by excessive heat and so treated with cooling remedies, resulted from wedge-shaped particles prying open the pores of the glands filtering urine in the kidneys.[29]

The programmatic mechanical anatomy and medicine of the seventeenth century drew on and drove original experiments and observations, generating explanatory and descriptive successes. It moved well beyond Descartes's speculative suggestions, including his core commitments to a mobile pineal gland and a passive heart. Anatomists mocked such claims as figments of the imagination opposed to anatomical observation.[30] Yet philosophers and physicians who rejected faculties and souls in explanations and ontologies remained committed to some form of mechanism as an *ideal* of explanation and ontology.

The mechanist and chymical strains of thought also interacted. By the middle of the seventeenth century, most natural philosophers, including mechanists, came to know the composition and properties of bodily substances through chymical analysis. Chymistry as an essential means for gaining knowledge of the body's composition and properties is significant in the work of Boyle, Malpighi, Thomas Willis, Hooke, Franciscus Sylvius (Franz Dele Boë), John Mayow, Richard Lower, and, later, Isaac Newton and Leibniz.[31] For the more committed mechanists, "fermentation" and other chymical processes came to be used as materialist placeholders for mechanical accounts yet to be provided.[32] Such promises could not always be fulfilled; the nature of many selective processes, such as glandular secretion, remained a mystery. Along with other challenges, most notably the profound

[28] Hooke (1665, 186). [29] Bertoloni Meli (2011, 314).
[30] Anstey (2000a); Bertoloni Meli (2011); Guerrini (2015); Ragland (2016).
[31] Frank (1980); Bertoloni Meli (2011); Smith (2011). [32] Bertoloni Meli (2011, 228).

difficulties in giving a mechanistic explanation of epigenesis,[33] these problems contributed to a "Newtonian" broadening of causal powers and a vitalist backlash against strict mechanism in the eighteenth century.[34]

Experience, Experiment, and Method in Renaissance Academic Medicine

Throughout our period, these various theoretical developments interacted complexly with methodological ones. Explicit discussions of proper method in attaining medical knowledge, as well as descriptions of how empirical resources could be produced in support of particular claims, were already prominent in the sixteenth-century tradition of learned medicine. The former included histories of medicine and descriptions of the ancient medical "sects" (e.g., Empirics, Dogmatists, and Methodists), in which authors usually followed Galen, highlighting the importance of combining experience and reason. The latter included complex anatomical procedures, collected case histories, and other reports of clinical experience, trials, tests, and experiments. Drawing importantly on these resources, early modern medicine was a fruitful site for the development of new categories and practices of observation and experimentation.

The practice of the ancient medical sect of the Empirics or Empiricists (*empeirikoi*) presented early modern physicians and philosophers with a stark version of empiricism and the original referent for the term.[35] Galen's *Outline of Empiricism* (*Subfiguratio empirica*) and other works offered systematic discussions of the Empirics' approach. Empirics dismissed the search for and claims to knowledge of hidden causes, practices characteristic of the Rationalists (*logikoi*) or Dogmatists (*dogmatikoi*). Empirics also rejected appeals to dissection-based accounts of the inner workings of the human body, and instead relied on the appearances (*ta phainomena*) through experience (*empeiria*) and memory. Their claims to medical expertise – as guide to what to do to effect a cure – rested on three sources: *empeiria*, *historia*, and imitation. They divided *empeiria* into three sorts of trial or *peira*: involuntary (chance or natural), voluntary (intentional), and imitative. In none of these is

[33] Indeed, the problem of animal generation was a catalyst for much interesting theoretical, observational, and broadly philosophical work in our period (Roger 1997; Smith 2006b; Blank 2010). A judicious examination of the history of work on generation and the philosophy of the Scientific Revolution covers an extensive primary and secondary literature and is, we regret, beyond the scope of this chapter.

[34] Roger (1997); Roe (2003). [35] Hankinson (1987); Pomata (2011b).

there a strong notion of testing general or theoretical claims, but rather of using whatever is at hand and then passing on and collating the results. *Historia* consisted in other people's reports that described the phenomena, without reference to theory. Imitation meant extending established expertise by using similar cures on similar diseases in similar patients. Thus, the "empiricism" of the Empirics aimed squarely at correlating the phenomena of health, disease, and curing through experience, memory, and sharing reports of proven remedies, without ever aiming to provide knowledge of causes, universals, or principles.

In addition to this ancient "empiricism," a rather wide range of other, sometimes quite different, methodological approaches can be gathered under a broad, multi-faceted notion of "empiricism." These include, for example, the insistence of Galen and most other pre- and early-modern physicians that the senses usually represent reliably the (real) qualities of things in the world; or the reliance (common among anatomists and botanists) on one's own perception (*autopsia*) over prior ideas, reports, or texts; or again, the later commonplace among Aristotelian philosophers and physicians that there is nothing in the intellect which was not formerly in the senses. Rather differently, we can also speak of the empiricism of the Paracelsian attention to technically manipulated phenomena of chymical changes, understood to reveal the deeper intellectual, spiritual, or semi-material agents behind sensory alterations. Different again is the empiricism of Santorio Santorio's use of instruments to measure the pulse, bodily heat, and changes in weight in the service of more certain medical practice. Informed by an ontology which denied the importance of the proper sensibles (hot, cold, wet, dry, etc.) in favor of the common sensibles (size, shape, local motion, etc.), Santorio's use of instrumentation to go beyond the appearances is distant from the "empiricism" of the Empirics, and closer to the very different practice and "empiricism" of his friend Galileo.[36] Let us examine several of these "empiricisms" in more detail.

From the end of the fifteenth century, scholars' and physicians' engagement with ancient texts and methods drove an elevation of personal experience over even ancient authorities. Already in the 1490s, controversies over the accuracy of Pliny's natural history, contradicted by other texts and physicians' own experience, led to increasing emphasis on personal sensation and experience of things, in conjunction with humanist textual scholarship.[37] Early modern academic physicians shaped and popularized the two epistemic

[36] Grmek (2008); Bigotti (2017). [37] Ogilvie (2006, 122–133).

categories of *historia* and *observatio*.[38] From the early 1500s, Galenic texts supplied a notion of *historia* as narrated *autopsia*, while Aristotelian tradition set out a distinction between *to hoti* ("the fact that"), description without causes, and *to dioti* ("the reason why"), knowledge of causes. By the second half of the sixteenth century, humanist physicians crafted a category of *historia* as autoptic descriptive knowledge preparatory to causal explanation. Seventeenth-century philosophers and physicians broadened the notion of *historia* to "a descriptive account of observational knowledge in any field."[39] In the 1560s, scholars' recovery of the ancient medical Empirics, together with the Latin translation of the skeptical philosophy of Sextus Empiricus (an ancient Empiric physician), shifted the meaning of *observatio*. Pushing aside its meaning as acting according to a rule, scholars instead elevated and popularized the term as a focused, personal attention and description of the "appearances" (*phainomena*).

In addition, sixteenth-century physicians increasingly made hands-on tests of phenomena and claims, especially in drug testing and anatomy.[40] From Galen through the seventeenth century, most physicians and philosophers agreed (at least in theory) that one had to make trials of drugs in order to know their properties. Even though a drug's faculties and actions in the body ought to work according to its temperament or mixture of elemental qualities (e.g., predominantly hot drugs should dry), this did not always work out in practice. For Galen, inferences to the faculties of drugs from the qualities of their ingredients had to be confirmed by the "qualified" experience of a trial (*peira*). Only this rational, methodical experience could control for interfering factors and isolate causes, and so really test whether foods or drugs had certain faculties. In this way, one could also make new discoveries. Influential texts ascribed to Galen, for instance, describe setting up parallel trials with roosters to test the efficacy of theriac, a purported antidote for poisons.[41] Developing Galen's rules, Avicenna articulated seven conditions for generating knowledge of a drug's medical properties from the "footpath" of experience.[42]

This early ideal of carefully tested rational inference carried into the early modern period. Scholarly engagement with texts and controversies over facts and explanations repeatedly drove physicians and philosophers to make empirical trials.[43] Increasing appeals to Neoplatonic or otherwise "occult"

[38] Pomata (2005); Pomata (2011a); Pomata (2011b). [39] Pomata (2011b, 67).
[40] Stolberg (2015); Ragland (2017).
[41] Rankin (2017), citing Pseudo-Galen, *On Theriac to Piso*.
[42] Van der Eijk (2005); McVaugh (2017). [43] Ragland (2017).

powers (causes hidden from the senses) and the wider publication of Paracelsian texts, starting in the 1560s, provoked additional controversies and recourse to hands-on trials, incorporating chymical concepts, equipment, and techniques. For example, Van Helmont proclaimed his reliance on revelatory dreams from God *and* quantitative chymical experimentation. Strikingly, both the dreams and the laboratory work aimed to isolate processes and control variables in order to discover causal relations.[44] Van Helmont distinguished between chymical experiments, which revealed the true *semina* and powers of things, and simple fire analysis, which artifactually produced the Paracelsian principles of mercury, sulphur, and salt as artifacts.[45] Attention to weight measurement in chymical processes – in the laboratory and in the living body – allowed him to argue for the conservation of matter in the growth of a willow tree and the analysis of urine.[46] He also moved beyond uroscopy to measure urine's specific gravity, arguing that changes in weight revealed real chymical processes, while color indicated only superficial accidents.

The sixteenth-century practice of making trials appeared increasingly in anatomy. From the 1520s and 1530s, the wave of newly available texts by Galen allowed anatomists to adopt and critique Galen's anatomical procedures and claims. In many cases, Galen's texts provided detailed models for making anatomical trials. In the 1550s and 1560s, the next generation of anatomists and physician-naturalists increasingly performed trials to test ancient or "modern" claims, or to establish structures, actions, and uses of the parts. Early in the 1500s, the humanist surgeon Berengario da Carpi used injections to test the action of the kidneys and fetal urine flow in order to adjudicate between contrasting theories: did the kidneys filter passively, like a sieve, or due to an active faculty; did the fetus expel urine through the umbilicus or the urethra? Similarly, Andreas Vesalius followed Galen in criticizing Aristotle's claim that the testes were not necessary for the production of semen, noting that he "made a trial" – carefully castrating a live dog – in order to test Aristotle's claim. His successor Realdo Colombo made trials of Galen's claim that finger bones had internal cavities and to see whether the pulmonary vein contained *pneuma* or blood. Colombo's student Hieronymus Fabricius ab Aquapendente identified tests that showed the function of the "little doors" (*ostiola*) or valves in the veins, and that established the transparency of the vitreous humor of the eye. In the former case, a chance discovery

[44] Ducheyne (2005). [45] Newman and Principe (2005, 84).
[46] Here Van Helmont likely followed the exemplar of a similar experiment in the writings of Nicholas of Cusa. See Pagel (1982a, 53–55).

of the slowing of the blood in the veins of an arm led to further tests of the action of the "doors"; in the latter, he denuded the humor of the eye and peered through it in order to adjudicate between different theories. Finally, Fabricius's student William Harvey discussed Galen's anatomical trials (which he called *experimenta*) in detail, and performed sophisticated procedures of his own to establish a revolutionary finding: the circulation of the blood. After Harvey's work on the circulation, physicians and philosophers increasingly made experimental tests on a wide range of animals, correlating observations of dead and living bodies, individual variations, ephemeral activity, and the sensitivity and complexity of living things.[47] Given the prevalence of physicians, including anatomists and chymists, in seventeenth-century scientific societies, the increasing status and widespread practice of making trials in Renaissance medicine surely informed seventeenth-century experimental philosophy.

Anatomical Method: Vesalius to Harvey

The presence of the practice of making trials in anatomy specifically can be situated within the broader methodologies of this newly prominent discipline. By the end of the sixteenth century, there were many chairs of anatomy in the medical schools, and their holders typically thought of themselves and published as *anatomists*. This distinct disciplinary identity reflected a slow, uneven, but real rise of importance of dissection in medical schools over the course of several centuries and the work, mentioned above, by medical humanists to recover works of Galen related to anatomy and dissection.

Galen praised his own books, but also demanded that students of anatomy "not only acquire accurate book-knowledge of each bone, but also examine assiduously with your own eyes the human bones themselves."[48] Long personal experience with all human body parts produced the best anatomical knowledge, with bodies of apes and other animals offering limited substitutes. The revival of anatomy beginning around 1300 embraced first private dissections in the form of post-mortem autopsies to determine the causes of death or identify signs of sanctity, then academic dissections demonstrating parts of a body according to a standard text.[49]

By the 1490s, anatomists and physicians, especially at Bologna and Padua, began reforming anatomy, using their own experience in dissections and humanist erudition to go beyond ancient texts, and attempting to understand

[47] Frank (1980); Bertoloni Meli (2011). [48] Galen (1956, 3). [49] Park (2006).

an apparently new disease, the French disease (syphilis).[50] Anatomists increasingly did their own dissections, made discoveries, and critiqued ancients and contemporaries. For instance, Berengario's 1521 book expressed his commitment to anatomical demonstration (*demonstratio*) according to the senses (*ad sensum*), which he used to teach, discover new anatomical structures, and adjudicate disputes in anatomical and philosophical texts.[51] E.g., he rejected Galen's identification of a network of blood vessels in the brain (the *rete mirabile*) for the elaboration of psychic spirit. Similarly, Niccolò Massa's 1536 book describes his own findings from dissections, claiming to deal with the anatomical "thing itself" (*res ipsa*) rather than names or books. He noted errors in Galen, Avicenna, and other authors, explicitly claiming to reject the opinions of authorities where they contradicted the senses, and to follow not a medical sect but "sensate truth" (*sensata veritas*).[52] As with Berengario, such "sensate truth" came from long experience in the dissection of a range of human and animal bodies. Although criticism of particular points in Galen surface, much of this work was also understood explicitly as a recovery of Galen's practice as exemplified in newly recovered or translated works.

While it is important to place it within this longer history, it is still accurate to see Vesalius's *De humani corporis fabrica* (1543) as pivotal in the rise in prominence of a self-confidently empirical anatomical tradition. The lavishly produced book quickly became an important point of reference in anatomical publications. Key features of the work included the number, size, and quality of anatomical drawings; their effective integration with the extensive descriptions of the human body; an easy mastery of the recently recovered anatomical texts of Galen; an unprecedently wholesale (though still somewhat muted) criticism of the same; and the prominent insistence on the need for and efficacy of human dissection (and animal dissection and vivisection) for producing medical knowledge. In its prefatory dedication, Vesalius frames the *Fabrica* as the culmination of his efforts to restore anatomical science to its ancient, pre-Galenic, Alexandrian glory – an effort that requires physicians to devote sustained effort to the manual work of human dissection. He argues that Galen had almost no experience of human anatomy, having dissected only animals, especially apes. Thus, Galen's work is full of errors; in a single anatomical demonstration, Vesalius reports, he can point out more than 200.

At points, Vesalius recognizes that his discoveries not only correct widespread errors in structural anatomy, but also provide evidence undermining important functional claims in physiology. While he shies away from these

[50] Lind (1975). [51] French (1985). [52] Massa (1536, 106[r]).

issues, he cannot avoid them. Though his *Fabrica* is dominated by structural descriptions, Vesalius always places them within a wider theoretical framework. As he writes in the dedicatory preface, even in public dissections, the anatomist goes beyond mere description of the parts to discuss their actions and uses. Vesalius devotes the last chapter of the *Fabrica* to vivisection, which he says (paraphrasing Galen's *De usu partium*) is especially effective in revealing action and use, and recounts a series of vivisectional demonstrations.[53] Throughout the *Fabrica*, when treating a part, Vesalius defines it, describes its variations, and identifies its actions and uses. Though he regularly follows Galen on action and use, he is aware of potentially important problems for these views. In the dedicatory preface, Vesalius describes the subject of his work as "anatomical science," the "science of the parts of the body," a "branch of natural philosophy," and "foundational to medicine." The emphasis on action and use (moving beyond mere description to causal considerations) undergirds these claims. For Vesalius, anatomy is a systematic, empirically-based scientific discipline, and his *Fabrica* constituted a forceful argument for the value, possibility, and indeed the actuality of an empirical, dissection-based research program for the discovery of new truths about the human body.

Fabricius, taking up Vesalius's position at Padua a few decades later, embraced and developed these basic epistemological and methodological principles. Fabricius recognizes the accomplishment of Vesalius, but insists that much new is still being discovered, and criticizes him for not attending adequately to more important concepts of action and use, which identify the final causes of the parts. Somewhat late in his long career,[54] Fabricius published in quick succession a series of anatomical studies focused on different functionally-defined organ systems. In the opening to one of the first, *De oculo, visus organo* (1600), Fabricius writes,

> dissection (if one judges correctly) has this use: to manifest those things that belong to the eyes, that is structure, *historia*; to lead to the notice of the action and faculty; and ultimately to uncover and reveal the utilities of the eye.[55]

[53] The special relevance to action or use of several of these is left quite unclear (Shotwell 2013; Shotwell 2016).

[54] For an introduction to Fabricius see Adelmann's introduction in Fabricius ab Aquapendente (1942).

[55] Fabricius ab Aquapendente (1600, 1).

Fabricius's descriptive structural knowledge of the parts of the body – including discoveries of previously unnoticed structures like the valves in the veins – comes from dissection. Furthermore, he expects this dissection to lead one also to final causal knowledge of the part in terms of action and use.

Fabricius's use of animals in anatomy differs importantly from Vesalius's. For Vesalius, animals serve as helpful teaching and learning tools, because of their availability and the possibility of vivisection. Their value is subordinated to the goal of understanding human anatomy, and as Galen's errors showed, overreliance on them could lead the investigator astray.[56] Fabricius, in contrast, takes animal dissection to be crucial because his *subject* is the *fabrica* of all animals, not just humans. He says his anatomical works constitute a *Theatrum totius animalis fabricae*. This understanding is reflected in his treatises, which frequently examine what animals have the organ of interest, what systematic patterns of variation are exhibited across these animals, and how these are correlated with other differences. This correlative work guides his inferences to part action and use. This project, modeled on Aristotle's animal books, has been called his "Aristotle Project" and makes clear the extent to which Fabricius conceived of his project as scientific.[57]

Harvey's medical degree from Padua bears Fabricius's signature, and he was deeply influenced by his anatomy teacher. Harvey understands anatomy to pursue final-causal knowledge of the parts (expressed in terms of action and use) by means of dissection and ocular inspection. Like Fabricius, Harvey signals a zoological scope in the titles of his two major works, *De motu cordis et sanguinis in animalibus* (1628) and *De generatione animalium* (1651). And as in Fabricius's "Aristotle Project," this conception of the subject matter of anatomy is wedded to a comparative methodology designed to facilitate insight into the teleological structure of parts and their variations across instances. Intriguingly, in a set of notes for dissection-based lectures, Harvey twice references a "Rule of Socrates" – once when setting out basic "canons" of anatomy, and once in the treatment of the function of a particular organ. By the "Rule of Socrates" Harvey seems, in the first instance, to have in mind Socrates' suggestions in the *Republic* that justice may be more easily discerned in the state than in the soul, and that agreement between accounts of justice in the state and in the soul should increase confidence in their truth. Similarly, thinks Harvey, the action and use of parts are often discerned more easily in some animals, and confidence comes when insight into the function of a part in one context leads to and

[56] Shotwell (2016). [57] Cunningham (1985); Distelzweig (2014a).

harmonizes with an account of function in others. Thus, universal, teleological knowledge is pursued by correlating anatomical structure across animals, guided especially by the most conspicuous cases. This methodology shapes both *De motu cordis* and *De generatione animalium*.

Another more widely acknowledged feature of Harvey's methodology is a kind of interventionist vivisection, drawing on the already recognized value of vivisection in anatomy and the practice of making trials traced above. In *De motu cordis*, Harvey uses vivisection not only to view parts in motion, but also to intervene on the anatomical systems of living animals.[58] In these wide-ranging interventions, Harvey aims to artificially produce new physiological contexts providing suggestive observations more or less directly supporting his account of the motion of the heart, arteries, and blood. As such, they seem of a piece with his broader comparative methodology, searching across different contexts for conspicuous cases, guided by the Rule of Socrates. Regardless, and in contrast to, e.g., Vesalius's chapter on vivisection, the evidential significance of the various vivisectional interventions is much more clearly articulated.

Beneath his comparative and interventionist methodology, we see in Harvey the basic Aristotelian "empiricist" principle, "Nothing in the intellect which was not first in the senses."[59] Echoing earlier anatomists, Harvey famously asserts that he both learns and teaches anatomy from nature, not from the books of philosophers. He also states that in order properly to attain such knowledge, each individual must undertake such observation. *Autopsia* – seeing for oneself – grounds *scientia*. Drawing on passages from Aristotle that touch on the dependence of *scientia* on the senses, Harvey insists that without appropriate sense experience one cannot properly understand or judge anatomical claims. Harvey is not naïve concerning the difficulty of dissection and the obscurity and ambiguities encountered in any particular specimen. He insists that to attain anatomical knowledge one must undertake many dissections, continually returning to new specimens to check, confirm, or correct one's claims. Such repeated dissection both leads to and requires considerable anatomical expertise. In numerous places Harvey uses *experientia* to refer to such experientially acquired anatomical expertise.[60]

A prominent view in the literature connects Harvey's empiricism with his insistence that he has established the systematic circulation, despite not

[58] His use of intervention extends beyond vivisection: he uses tourniquets to intervene on a living human without vivisection.

[59] Aquinas, *De veritate*, q. 2 art. 3 arg. 19. [60] Goldberg (2016).

providing the use (final cause) of that circulation. According to this view, Harvey has thereby elevated non-causal, non-explanatory, empirical fact to the level of knowledge in its own right.[61] Though highlighting important aspects of Harvey's work, such a view would be misleading if it was taken to imply that Harvey rejected the Aristotelian distinction between establishing a descriptive claim that something is the case (*to hoti*) and attaining causal knowledge why it is the case (*to dioti*), or that he rejected the search for final causes, or that he thought empirical research terminated in mere description. For Harvey, that the final cause of the circulation is not established in *De motu cordis* reflects the incompleteness of his anatomical research.[62] That the circulation can be established without identifying its final cause reflects for Harvey the structure of anatomical investigation, which allows one to establish (in fact, in which it is typical for one to establish) that something is the case before establishing why it is. That unexplained facts can be established without causal knowledge, points out Harvey, is presupposed by the long-standing genre of "disputed problems" in which competing causal explanations of a given fact are examined and evaluated.[63]

Anatomical research in the wake of *De motu cordis* bore the marks of Harvey's successes. Harvey's tightly argued piece sets the stage, providing both a host of research questions (by undermining vast swaths of physiology) and something like a method or model of investigation. Harvey's use of vivisectional interventionism was highly suggestive and was employed by many by the mid-seventeenth century, receiving additional prominence with the rise of "experimental philosophy."

Conclusion

Over the course of our period, the medical science of the living body saw a focused development and expansion of experiential and experimental methodologies (both ideals and practices) inherited from early sixteenth-century learned medicine interacting also with developments in chymistry and the rising experimental philosophy. There developed a growing confidence and rhetorical insistence that such ideals and practices led and would continue to lead to new discoveries and insights. In addition, there were important transformations in (and constrictions of) the explanatory resources that could be deployed to understand the living human body. Broadly Galenic and Aristotelian approaches to physiology were displaced by new

[61] Wear (1983); French (1994). [62] Distelzweig (2014b). [63] Harvey (1649, 97).

efforts drawing on chymical and mechanistic ideas. Throughout the Scientific Revolution, these theoretical and methodological developments interacted in complex and productive ways. Indeed, it is perhaps best to see medical efforts to develop the science of the living body in this period as an exploration of the changing space of possibilities defined by varying theoretical commitments and a broadening commitment to, expectation of, and attention to discovery by experience and experiment.

12

Experimental Natural History

PETER R. ANSTEY AND DANA JALOBEANU

Introduction

Today the term 'natural history' conjures up ideas of butterfly collections, watercolor depictions of plants, and endless classifications (in Latin). These, however, are the wrong images to bring to the subject of what was, in the seventeenth century, a complex, dynamic, collaborative, and experimental enterprise. This chapter aims to tell the story of its emergence and impact. It is a story of the transformation of natural history from a science of collecting, describing, and classifying to a complex discipline whose aim was to investigate, in an orderly manner and with the help of the most up-to-date scientific instruments, all natural phenomena, both above and below the crust of the Earth.

This experimental natural history was an architectonic approach to the assembly, ordering, and analysis of phenomena. It was embedded in a particular theory of knowledge and it came pre-loaded, so to speak, with a philosophy of experiment. Once it was applied and developed, it led to the emergence of new sciences, new genres of natural philosophical writing, and, above all, to a new vision of what sort of human effort and what kinds of institutions were required to achieve its goals.

This chapter begins by setting out the broad contours of the new historiography of natural history that has emerged over the last two decades and that, in some respects, still remains rather fluid. It then turns to the nature and roles of experiment within experimental natural history and an analysis of the philosophical dimensions of experimental natural history as a knowledge-generating enterprise in natural philosophy. Then, moving from these more abstruse topics, it deals with the practical application of experimental natural history, both as a driver of the emergence of new sciences and as a way of organizing and motivating pre-existing sciences. Thus, in a general sense, the chapter moves from historiography to

philosophy and ends with history of science. As for chronological and geographical range, we begin with Francis Bacon, including a short backward glance at Renaissance natural history, and we end in the early eighteenth century. The geographical range of the chapter is predicated on the historiography presented here. In brief, experimental natural history was conceived in England and underwent its first theoretical and practical application there from the late 1650s. There were important mid-century echoes on the Continent, especially in France and Italy; however, far and away the bulk of its output and impact was in the British Isles.

The Nature and Emergence of Experimental Natural History

Much has been written in recent years about the history of natural history, and much remains to be investigated. To date, there are several main directions of research. Some see Renaissance natural history as emerging from the medieval encyclopedic tradition, as a science of particulars. Others regard the emergence of natural history as a novel discipline arising from debates and transformations surrounding the *materia medica*. There are numerous discussions of the nature of such transformations and the status of the resulting discipline, which some classify as a new epistemic genre, and others, a form of *scientia*.[1] All scholars agree, however, that natural history is one of those branches of knowledge that underwent important developments during the Renaissance.

By the end of the sixteenth century, naturalists found themselves inhabiting a larger and richer world than the one described by the ancients, in which both the natural borders and the disciplinary boundaries had become fluid and imprecise. They had to identify and name new specimens, re-think taxonomies, and consider new systems of classification. In this work of describing and recording, not only did they attempt to re-evaluate and update the available ancient sources,[2] they also had to reconsider the traditional

[1] A selection of relevant sources to illustrate the diversity of this landscape would include Maclean (2005); Pomata (2005); Pomata and Siraisi (2005); Findlen (2006); Ogilvie (2006); Pomata (2010).

[2] On one interpretation, Renaissance natural history became an autonomous discipline precisely as a result of successive attempts to harmonize the works of reform and *renovation* that took place in the world of learning, spurred on by new observations and the newly acquired richness of the natural world. Already at the end of the fifteenth century, some humanists were at work correcting and updating Pliny's *Historia naturalis*, and some of these efforts continued into the sixteenth century. For discussion, see Lestringant (1994); Findlen (2006, 442).

(Aristotelian) relation between natural history and natural philosophy.[3] Some, at least, were willing to claim for their investigations a certain degree of autonomy and a place in the university curriculum.[4] Thinking in disciplinary terms also raised questions of methodology, validation, corroboration, and collaboration among naturalists. As has been recently shown, the large field – or maybe the disciplinary cluster – of natural history saw the development of the first large-scale (sometimes Europe-wide) collaborative projects in areas classified today as botany, geography, and medicine.[5] By the turn of the century, we can see these developments gaining momentum. We can also see the emergence of at least two trends in natural history. We will call the first "classificatory natural history," to distinguish it from the other, "experimental natural history." The two are not mutually exclusive – in fact, some of the characters discussed in this chapter were interested in both – and we can see them evolving, in parallel or intertwined, until the beginning of the eighteenth century. However, it is important to distinguish them, and we think that a clarification of this distinction can serve as a good tool for mapping the diversity of natural historical approaches one can find in early modern science.

We can see both the classificatory and experimental natural history as originating in Francis Bacon's attempts to rework natural history into a foundation for all sciences. Natural history is, according to Bacon, the first of the three "true *stages of Sciences*" that, like the construction of "pyramids," are perched one upon the other.[6] This foundational approach is complex and multi-layered (and often insufficiently understood). For Bacon, natural history is foundational in at least two different ways. First, it provides natural philosophy with "materials for the building";[7] i.e., a storehouse of recipes, experiments, observations, and inquiries from

[3] In Aristotle, natural history can be seen as a preliminary requirement for the construction of a *scientia* properly speaking; from natural histories researchers can infer principles of new sciences. This is fairly clear to the reader of Aristotle's biological works. On the other hand, for readers focusing on physics and metaphysics, natural history is not a "scientific" discipline and does not need to be incorporated into the curriculum. One can see the popularity of natural history in the Renaissance not so much as a rejection of Aristotelianism, but as a recovery of Aristotle's "biological" works and their implicit methodology. See, for example, Perfetti (2000).

[4] See, for example, Kusukawa (1995); Perfetti (2000).

[5] On collaborative projects in the natural history of plants, see Ogilvie (2006); Schiebinger and Swan (2007); Egmond (2010). On geographical and natural historical collaborations, see, for example, Barrera-Osorio (2006); McLean (2007). On medical collaborations, see the illuminating discussion of medical practitioners, artists, engravers, and typographers in Kusukawa (2012).

[6] Bacon (1674, 103).

[7] As William Rawley puts it in the introduction to the *Sylva Sylvarum*; see Bacon (1859, 336).

which the investigator of nature can proceed to the construction of theoretical superstructures by *inductio*. Second, natural history also serves as a storehouse of methods and instruments of inquiry, theoretical observations, and hypotheses. In this sense, the border between natural history and natural philosophy is fully permeable. Moreover, natural history provides to natural philosophy the elements of what has been called a "philosophy of experiment."

If we take the first sense in which natural history is foundational, we can note that Bacon claims that what natural history has to offer in terms of "materials for the building" is comprehensive and organized. This is what Bacon in some texts calls a primary natural history (*historia prima*), or "mother" natural history (*historia mater*).[8] He gives precepts for organizing and assembling such a primary natural history and discusses two complementary ways of classifying phenomena in order to produce a well-organized natural history. One classification is according to the way phenomena are produced; and this divides them into "generations," "pretergenerations," and "arts." The other is according to a cosmological scheme, by the region of the universe in which phenomena appear. Its result is a list of 130 natural histories to be completed (presumably by his followers).[9]

This primary natural history is, however, just the first level of Bacon's reworking of natural history into a complex, hands-on, collaborative discipline. It is a provisional and dynamic attempt at classification, since Bacon is not in fact committed to any of the received divisions of natural kinds circulating in the Renaissance. He is also rather ambivalent about the possibility of distinguishing sharply between the natural and the artificial. As he puts it in one of his famous aphorisms,

> With respect to works, man can do nothing except bring natural bodies together or put them asunder; nature does the rest.[10]

If we take this and similar statements at face value, Bacon's classification of phenomena into generations, pretergenerations, and arts does not mean more than a functional grouping or practical division. Similarly provisional

[8] Bacon (2004, 452).

[9] Bacon's *Instauratio magna* (1620) is a volume composed of fragments of Bacon's large-scale plan: a general plan of the work (*Distributio operis*), the unfinished *Novum organum*, a methodological text on how to write a natural and experimental history (the *Preparative to a Natural History to Come*, or *Parasceve*), and a *Catalogue* of particular natural histories to be undertaken. The latter two are important for his proposed classifications of "materials for the building."

[10] Bacon (2004, 64, modified).

are his topics pertaining to natural histories of heavens, meteors, air, plants, animals, etc. He is also happy to work with multiple (provisional) classifications,

> since many experiments come under two or more titles . . . it may be best to investigate them with reference to arts but arrange the results with reference to bodies. For I care little for the mechanical arts in themselves, except insofar as they help in fitting out philosophy. But these matters are better managed as particular circumstances demand.[11]

Given such statements, one would expect Bacon to be less interested in taxonomies and the classificatory purpose of natural history. This, however, is not so. His natural histories are filled with attempts to classify processes and phenomena. Sometimes he begins with received classifications; as in the *History of Winds*, where he enumerates the traditional names of winds.[12] But received classifications are soon abandoned in favor of classes emerging from more sophisticated, experimental inquiries. Bacon calls the result of all this "natural and experimental history."

Natural and experimental histories are organized around units called "inquiries" (*inquisitiones*). Inquiries emerge by selecting, from the wide diversity of phenomena, a particular range of phenomena subject to further investigation with the help of observation and experiment, in a manner carefully regulated by a pre-established experimental methodology, or "art" of experimenting.[13] This art of experimenting, called *experientia literata* (literate or learned experience), codifies the manner of moving from one experiment to the next by the controlled variation of experimental parameters and by careful analogical extensions of experimental results into new fields and new directions.[14] With the help of this art of experimenting, ordered experimental series can be assembled into particular natural and experimental histories of such phenomena as "winds" (i.e., phenomena of dilatation, condensation, and "phase transition" through which water becomes vapor and vapors move masses of air in the atmosphere), "sound," "life" (i.e., pneumatized matter heated and "inflamed"), "heat and cold," "dense and rare," "heavy and light," etc. During his lifetime, Bacon managed to publish only two such natural and experimental histories, but he left a wide range of prefaces, introductions, and fragments of inquiries so that his followers could

[11] Bacon (2004, 485). [12] Bacon (2007, 18–21). [13] See Jalobeanu (2016b).
[14] In many ways, Bacon's *experientia literata* can be seen as regulating the natural analogical moves of the human mind, excluding unsound analogies and hasty generalizations. See Jalobeanu (2015); Jalobeanu (2016b).

reconstruct at least the broad lines of his general project (called *Historia naturalis et experimentalis*). He insisted that these natural and experimental histories can function as "exemplars" for researchers interested in following in his footsteps.[15]

And seventeenth-century naturalists did walk in his footsteps. The more confident pursued specialized, experimental inquiries, led by the "art" of moving from one experiment to another, in the direction of more sophisticated inquiries. Others, more modest or more diligent, set their goals lower, aiming at completing the primary, classificatory natural history.[16]

Although classificatory and experimental natural history were never mutually exclusive, they have quite distinctive features. The former works toward the establishment of order. It can assume a given order or accept a provisional order, but once the system of order is set in place, research (and especially experimental research) is subsumed to a classificatory purpose. Even when practitioners of this type of natural history were doing experiments, they did them with a view to correctly placing results in a general system of classification.[17]

By contrast, experimental natural history is driven by experimental inquiry. It is an open-ended, heuristic enterprise whose aim is (ultimately) the acquisition of new knowledge; and if the new knowledge thus obtained contradicts the "kinds" assumed at the beginning, the experiment prevails. We can almost say that the Baconian experimenter, by going where the experiments lead, is better equipped to penetrate to the underlying order of the world, constantly modifying his concepts to suit his results.[18] As a result, experimental natural history is fundamentally intertwined with natural philosophy.

Both classificatory and experimental natural histories were conceived as large-scale enterprises by Bacon's followers; and one can see them as a driving force behind putting together societies, committees of experimental

[15] Rees (2007); Jalobeanu (2015).

[16] Some also believed that Bacon's primary natural history could be harmonized with the more traditional classificatory natural history divided according to some sort of natural kinds. The diversity of seventeenth century Baconianism(s) still needs to be investigated. See, for example, Giglioni (2013); Jalobeanu (2015); Jalobeanu and Matei (2020).

[17] Compare, for example, Robert Boyle's *Memoirs for the Natural History of Humane Blood* (1684) with Robert Morison's *Plantarum historiae universalis Oxoniensis pars secunda* (1680) to see the sharp distinction in purpose, function, and style between a primarily experimental and a classificatory enterprise. Meanwhile, it is worth noting that there were interesting attempts at classification that resulted from experimental natural history. One example is Robert Sharrock's classification of plants according to whether they can survive on water only. For a discussion of Sharrock's experiments, see Jalobeanu and Matei (2020).

[18] One example is Bacon's multiplications of motions and processes to account for the diversity of phenomena. See, for example, Jalobeanu (2018).

investigation, scientific publications, and other forms of organizing and recording knowledge. Experimental natural history was also seen as essentially open-ended. As Joseph Glanvill put it in a letter to the Duchess of Newcastle, Margaret Cavendish, in 1667:

> all that we can hope for, as yet, is but the History of things as they are, but to say how they are, to raise general *Axioms*, and to make *Hypotheses*, must, I think, be the happy privilege of succeeding Ages; when they shall have gained a larger account of the *Phaenomena*, which yet are too scant and defective to raise Theories upon: so that to be ingenious and confess freely, we have yet no such thing as Natural Philosophy; Natural History is all we can pretend to; and that too, as yet, is but in its Rudiments.[19]

It is this conception of experimental natural history, clearly deriving from the writings of Francis Bacon, that came to play a central role in seventeenth-century natural philosophy. Dividing Bacon's grand plan into workable chunks – natural and experimental histories – went hand-in-hand with selecting from Bacon's writings elements to be taken over and elements to be rejected, replaced, and corrected. This is how experimental natural history gradually came to be associated with the emergence of new arts and sciences. One can see this already at work in Marin Mersenne's attempts to develop a science of sounds (a form of mixed mathematics with a substantial experimental component).[20] In Britain, a form of natural history focusing on horticulture and deriving from Bacon's *Sylva Sylvarum* was developed among the members of the Hartlib Circle during the 1650s, and continued into the 1670s.[21] In the second half of the seventeenth century, experimental natural histories proliferated, and Bacon's methods of investigation began to be applied in chymistry and pneumatics in the work of Robert Boyle, as well as in the investigation of processes of freezing, rarefaction, condensation, petrification, etc., in the context of the early Royal Society.[22]

[19] Anonymous (1676, 124).

[20] On the relationship between Bacon and Mersenne, see Buccolini (2013); Jalobeanu (2014).

[21] See Matei (2017); Jalobeanu and Matei (2020).

[22] To take freezing as an example, see Boyle's *Cold* (1665), which also contains an account of freezing by Christopher Merrett that was read to the Royal Society in 1663 (Boyle 1999–2000, 4:525–542), and the account of William Croone's experiments read before the Royal Society in February 1683 (Birch 1756–1757, 4:263–264). In his preface to *Philosophical Transactions* 6 (1670), Henry Oldenburg gives a brief account of the "parts of natural philosophy which are now advancing" and their "applications." The list includes new fields of investigation in optics, mathematics, and mechanics, but also "vegetables brought under a philosophical consideration," "inquiries into the properties of Springs; of Subterranean steams and Liquors," etc. (Oldenburg 1670b, 1149–1150).

The continental reception and practice of Bacon's experimental natural history was perhaps more nuanced, although much remains to be investigated. There is no doubt that Bacon's natural historical works were read and regarded as innovative by many, including Isaac Beeckman, Marin Mersenne, and René Descartes.[23] By the 1660s, in the lead-up to the formation of the Parisian Académie Royale des Sciences, Christiaan Huygens had cause to recommend a broadly Baconian plan for the Académie to Jean-Baptiste Colbert.[24] Meanwhile, Henry Oldenburg continued to promote the cause through his extensive correspondence network and a number of leading English writings promoting experimental natural history, and some of the English natural histories, were translated into French.[25] Yet for the French natural philosophers, there was a less clear-cut divide between the experimental and the classificatory purposes of natural history; and, in general, French natural history remained closer to the project of classificatory natural history.[26]

Experimental Natural History and the Philosophy of Experiment

Mere experimentation, Bacon claims, is nothing but a sort of "groping in the dark."[27] Proper experimental investigation requires an "art" of experimenting made of methodological precepts and heuristic rules that, according to Bacon, are the metaphorical equivalent of "being led by the hand" through the labyrinth of nature.[28] But how is all this supposed to work? In the second half of the seventeenth century, naturalists began to question and discuss the philosophical aspects of Bacon's art of experimenting. And this happened in a particular context: the emergence of experimental philosophy. This was a movement within natural philosophy with its own methodology, practitioners, rhetoric, opponents, and literary and material outputs. Experimental philosophy emerged in the 1660s and was established first in the early Royal

[23] See Georgescu and Giurgea (2012); Gemelli (2013); Jalobeanu (2014).
[24] See Huygens (1888–1950, 6:95–96).
[25] See, for example, Oldenburg (1965–1986, 2:141–144). See also Childrey (1667); Sprat (1669).
[26] See Anstey (2018).
[27] Bacon (2004, 159). For Bacon's criticisms of "blind" experience, see Jalobeanu (2019, 24–25).
[28] As he puts it in *De augmentis scientiarum*: "when he uses some direction and order in experimenting [*cum vero nonnulla utatur in experimentando directione et ordine*], it is as if he were led by the hand; and this is what I mean by Learned Experience [*experientia literata*]" (Bacon 1857–1874, 4:413).

Society of London, spreading quickly to Italy and later to the Netherlands and France.[29] The first generations of experimental philosophers appropriated, developed, and applied Bacon's method of natural and experimental history.[30] They were its chief theorizers, practitioners, and defenders. But they also discussed its methods, developing it into something more sophisticated and more philosophical. In this section, we take a brief look at some of these more philosophical discussions, showing how they set the stage for a sophisticated philosophy of scientific experimentation.

Mapping New Fields of Knowledge: Queries and Articles of Inquiry

Bacon's inquisitions are organized with the help of articles of inquiry; i.e., questions or set-problems, either theoretically postulated or experimentally established. Bacon did not invent this way of writing,[31] but he clarified and developed it, making it available as a tool for subsequent generations of naturalists. In this view, the way to begin a natural and experimental history is to create, within a given topic, a subsidiary list of queries that map out the field of research. Thus, for a "history of man's humours," Bacon establishes the following articles of inquiries: "blood, bile, sperm."[32] Among John Locke's papers we find a series of titles for a natural history of human blood, and in 1684 Boyle published *Memoirs for a Natural History of Human Blood*, which itself is built around a set of queries and is dedicated to Locke.[33] Other sets of draft queries by Boyle never found their way into print; but an analysis of the process of evolution among these queries sheds light on how they were used to shape and direct the structure and content of the published history.[34]

In addition to setting the limits of a new field of inquiry, queries and topics of inquiry have heuristic functions: they direct the experimenter, they are effective in selecting data from books or from travel reports, and they are used to test (and further investigate) hypotheses and theories.[35] Thus, we can see them mapping new fields of investigation.

[29] Anstey and Vanzo (2012). [30] Anstey (2014); Jalobeanu (2015, ch. 8).
[31] See, for example, Edwards (1969).
[32] This is title 45 from the *Parasceve* (Bacon 2004, 479).
[33] See Boyle (2005, 19–20). For Locke and natural and experimental history, see Anstey (2011b, ch. 3).
[34] On the role of queries in experimental natural history, see Knight and Hunter (2007); Anstey (2014).
[35] We can see these aspects already in the queries and topics of inquiry formulated by Francis Bacon; for example, in *Articles of Inquiry into the Nature of Metals and Minerals* or *Inquisitio de animato et inanimato*. See Jalobeanu (2016b); Jalobeanu (2018).

According to Oldenburg, one of the main achievements of the early Royal Society, and the focal point of the first volume of the *Philosophical Transactions*, was to provide

> Enquiries and Directions for all Travellers by Sea and Land . . . . [T]o review and return a safe Testimony of all such Observables of Nature and Excellencies of Art, as carry the greatest fame, or seem most considerable for Use or Instruction. We have furnisht particular Inquiries for Mines, for Seas, for Springs . . . .[36]

This view of queries and topics of inquiry gives us a better understanding of the extensive interest in and use of travel literature by those practicing experimental natural history.[37] But we can also see the queries and articles of inquiry as playing an important role in the philosophy of experiment, as seen in the following.

Hypotheses and the Types of Experiment

Bacon and his followers, especially Boyle and Robert Hooke, had a distinct anti-speculative attitude.[38] They recommended a detached overview of current hypotheses, but cautioned against strong theoretical engagements.[39] Adopting a doctrine, or "owning a hypothesis," they claimed, would restrict the investigation, resulting in biased conclusions, ungrounded speculations, or unjustified simplifications.[40] Meanwhile, experimental philosophers displayed a keen awareness of the complex interplay between theory and practice. They tried to distinguish between queries, hypotheses, articles of inquiries, and doctrines; between well-established axioms

[36] Oldenburg (1669, 897). The early issues of the *Philosophical Transactions* contain many lists of queries for third parties, and the genre continued up to the end of the seventeenth century. See also Oldenburg (1965–1986, 2:142–144). For a later perspective on the same matters see Woodward (1696).

[37] See Carey (1997); Anstey (2011b, 59–61).

[38] For Bacon's anti-speculative attitude, see Jalobeanu (2015); Jalobeanu (2019); see also the *Epistle Dedicatory* to Boyle in which Oldenburg praises Boyle's anti-speculative attitude and his "impartiality" toward hypotheses (Oldenburg 1670a). For Hooke's anti-speculative attitude and his attack on the "fine dreams of Opinions" see his Preface to *Micrographia* (1665).

[39] See Hooke's *General Scheme* (Hooke 1705, 19, 65) and *Things Requisite to a Natural History* in Oldroyd (1987, 158). See also Boyle (2008, 2). Hooke's method has been described as one of "multiple working hypotheses" (Oldroyd 1972, 124). For a more general discussion, see Ducheyne (2013).

[40] On the Royal Society's aversion to hypotheses, see, for example, Sprat (1667, 62, 101). See Hunter and Wood (1986); Jalobeanu (2011). Following Bacon, Thomas Sprat and other *virtuosi* cautioned against the moral and epistemic dangers of "speculation." See Sprat (1667, 103); Jalobeanu (2015, ch. 3).

and mere hypothetical results;[41] and sometimes also between operational and explanatory terms and definitions.[42] They were not always entirely successful. Both specific inquiries and attempts to map new exploratory fields were often related to natural philosophical and metaphysical theories, but such theoretical engagements were often provisional and, at least in theory, subject to amendment by experimental feedback.[43]

Meanwhile, experimental philosophers recognized the diversity of epistemic functions performed by experiments.[44] They realized that experiments can do many things, and sometimes attempted to tie natural history to theory via determinate types of experiment, thus establishing "patterns" and "exemplars" of experimental investigations.[45] Indeed, seventeenth-century experimental philosophy abounds in typologies of experiments. Some originate in Bacon's distinction between luciferous experiments leading to a knowledge of the underlying causes of phenomena, and fructiferous experiments leading to useful knowledge.[46] Others originate in the distinction proposed by Bacon in the *Sylva Sylvarum* between solitary experiments and "experiments in consort."[47] Experiments in consort figure prominently in Nehemiah Grew's *Experiments in Consort of the Luctation Arising from the Affusion of Several Menstruums upon all Sorts of Bodies Exhibited to the Royal Society*. This work is advertised by Grew as "a Specimen of a Natural History of the Materia Medica."[48] It provides a record of a series of chemical experiments on animals, vegetables, and minerals, and is very self-consciously working with Bacon's typology of experiments.

Another type of seventeenth-century experiment is what Bacon calls in the *Novum organum* "crucial instances" – experimental tests able to decide between two hypotheses. Under the name 'crucial experiment', this

[41] We can clearly see such distinctions drawn explicitly in Isaac Newton's first paper on light and colors (Newton 1672). See Jalobeanu (2015, ch. 8).
[42] See, for example, Jalobeanu (2016a); Jalobeanu and Matei (2020).
[43] See Sprat (1667, 257).
[44] For discussions of examples, see Jalobeanu (2010); Dobre (2013a); Dumitru (2013).
[45] In the preface to *Philosophical Transactions* of 1669, Oldenburg depicts the achievements of the Royal Society thus: "we then made an Attempt of laying some Foundation for the Improvement of real Philosophy . . . ; in publishing Advices and Directions for the writing of an Experimental Natural History; in pointing out Essays, Patterns and Exemplars, that have hitherto been designed in that kind" (Oldenburg 1669, 894).
[46] See Bacon (1859, 336); Bacon (2004, 113, 157–159, 181). This typology is widespread amongst the members of the Royal Society; e.g., Boyle (1999–2000, 6:433–444).
[47] Experiments "in consort" are experimental series. This distinction provides one of the organizing principles of Bacon's *Sylva Sylvarum*. See Jalobeanu (2016b).
[48] Grew (1678, sig. $A4^{v}$).

particular form of experimental investigation will have a remarkable career in the seventeenth century and beyond.[49]

There were other types of experiments in the Baconian typology, including unsuccessful experiments, succedaneous experiments, loose experiments, and paralipomena. Two types of experiment that were not explicitly theorized but were used in the period are what are now called exploratory experiments and thought experiments. As it happens, Boyle used the term 'exploratory experiments' in his *Medicina hydrostatica* (1690),[50] though his papers on the subject are no longer extant. And there is no doubt that he performed exploratory experiments; that is, experiments that are not primarily determined by theory, but whose epistemic goal is to examine phenomena through the systematic variation of parameters of, say, a particular instrument or material quality. Boyle's air pump experiments in *Spring of the Air* (1660) provide good examples. In fact, there is a case to be made that exploratory experiments were particularly conducive to the experimental philosophers' predilection to avoid prepossession and being over-influenced by theory in the construction of natural histories.

As for thought experiments, while to our knowledge it was never listed in the Baconian typology of experiments, Boyle does speak in his *Designe* of fictitious experiments, which, to all intents and purposes, look like thought experiments:

> fictitious Experiments, wherein Processes and other wayes of Operating are propos'd to supply the defect of real Experiments, when we want them to determine doubts, to resolve Questions or for other purposes.[51]

Finally, it has not gone unnoticed that a number of leading early modern philosophers spoke of introspection as a form of experiment. This was certainly the case for Boyle, who says,

> for an experimental knowledge of several things, that highly concern us, we need, neither mathematical instruments, nor chemical furnaces, but only a due attention to those things, that are transacted within our own selves, and make impressions on us, whereto we can scarce avoid the finding of ourselves conscious.[52]

Of course, the leader here was Locke who, in *An Essay concerning Human Understanding*, frequently asks the reader to experiment within themselves. No doubt it is this rather loose sense of the term 'experiment' that would later

[49] See Dumitru (2013). [50] Boyle (1999–2000, 11:204). [51] Boyle (2008, 5).
[52] Boyle (1999–2000, 12:508).

incline commentators on Locke's *Essay* to call it an experimental history of the soul,[53] and it is what David Hume refers to in the subtitle of *Treatise of Human Nature*; namely, that the book is "an Attempt to Introduce the Experimental Method of Reasoning into Moral Subjects."

The Fate of Experimental Natural History and the Emergence of New Baconian Sciences

Proponents of experimental natural history, like most of their contemporaries, were committed to the Aristotelian view that all "scientific" knowledge (*scientia*) had to be derived from first principles. And while there were different views on how exactly we get epistemic access to the principles, the object of experimental natural history was, as both Glanvill and Hooke put it, "the Raising of Axiomes" from which demonstrations could be made.[54] It was in this sense that natural history was to provide the foundation "to superstruct, in time, a solid and usefull Philosophy upon."[55] There was, however, a problem. After many decades the proponents of this method of natural history had produced rather meager results when it came to the discovery of new principles. As Hooke puts it in his *Discourse of Earthquakes*:

> tho' the things so collected may of themselves seem but like a rude heap of unpolish'd and unshap'd Materials, yet for the most part they are so qualified as that they may be fit for the beginning, at least of a solid, firm and lasting Structure of Philosophy.[56]

By the 1690s, another approach to experimental philosophy had appeared on the scene: one which claimed to have discovered new principles from which knowledge could be generated. This was, of course, the mathematical method of Isaac Newton's *Principia* (1687). It is most likely that the appearance and subsequent success of Newton's mathematical method of natural philosophy was one of the reasons for the gradual demise of experimental natural history as a "research program" in natural philosophy in the early eighteenth century. The story of its fading from view is a complex one and cannot be told here.[57] Nevertheless, it does seem that, in spite of its failure to produce new principles on which to found sciences, the program of experimental natural history did make a unique contribution to the development of early modern science.

[53] For Locke, see Hamou (2019); for Laurence Sterne, see Anstey (2009).
[54] Anonymous (1676, 124); Oldroyd (1987, 158). [55] Oldenburg (1965–1986, 3:87).
[56] Hooke (1705, 329). [57] See Anstey (2014, 123–124); Anstey (2015).

While we cannot argue for the claim in detail here, we contend that the grand project of experimental natural history provided a kind of incubator for sciences that required the accumulation of observational and experimental evidence. The program stimulated and legitimated extensive experimental investigations in diverse fields that had lacked a common background theory, and sometimes also a well-defined methodology of investigation. In some cases, such as medicine, the calls to construct natural histories of disease from the likes of Thomas Sydenham and Giorgio Baglivi had few long-term consequences.[58] However, in various other fledgling disciplines, experimental natural history enabled practitioners to prime the pump, so to speak, by establishing an observational basis for their field. These include geology, the study of fossils, meteorology, and even the study of antiquities. By the time the architectonic plan of experimental natural history had begun to fade, this clutch of new disciplines were sufficiently established to become independent of the incubator.

Perhaps the best way to view this is in terms of the nested disciplines of the history of the Earth, geology and the study of fossils, regional histories (chorography), and the study of antiquities. Bacon had set out the contours of a history of the Earth in the *Parasceve*. These included what we might label macro-histories, such as "history of the earth and sea" and "geographical natural history." The "history of metals," fossils, and gems was included under the heading of histories of particular species. Everything pertaining to the nature of humans and their cultural outputs fell under "the history of man." Bacon's desiderata were adopted by some of the leading Fellows of the early Royal Society, and there is copious evidence of the quest to establish experimental natural histories of the Earth, including country histories and the more focused histories of species. This can be found in the *Philosophical Transactions*, the correspondence of Oldenburg, and the publications of Boyle. A key text is Boyle's *Heads for a Natural History of a Countrey Great or Small* (1666), which was republished nearly three decades later in 1692. Boyle also contributed a history of gems.[59] Hooke set to work on fossils.[60]

Recent work on the origins of geology shows that significant developments occurred in the 1660s in the writings of Boyle, Nicolas Steno, and Hooke, and that theoretical speculations concerning the process of petrifaction combined naturally with schemes for mapping geological formations and categorizing minerals and metals, all of which fell under the rubric of the Baconian program.[61] Geology,

[58] Baglivi (1696); Sydenham (1848, 12–15); Klein (2003); Anstey (2011a).
[59] Boyle (1672).
[60] Hooke's opinions on fossils were recorded in *Micrographia* (1665) and work on fossils is prominent in his *Discourse of Earthquakes* and other fragments published posthumously. See Hooke (1705) and also Kusukawa (2013).
[61] Yamada (2009).

so we would argue, had its origins in experimental natural history. So too did two cognate disciplines. The history of the Earth retained an overtly natural historical strand into the eighteenth century in the writings of Hooke, John Woodward's *Natural History of the Earth* (with its explicitly Baconian Preface), and in Buffon.[62] Similarly, chorography, which was reinvigorated along Baconian lines by Robert Plot, had within its compass the study of antiquities, an arrangement fully justified under the Baconian program.[63] This is the explanation of the occurrence of numerous articles in the *Philosophical Transactions* on Greek and Roman antiquities.[64] By the time of the establishment of the Society of Antiquaries in 1751, the study of antiquities had largely severed its links with natural philosophy, but the decades of incubation within the Baconian program had ensured its launch as a discipline in its own right.

The most revealing and important example, however, is meteorology. To be sure, the study of meteors had been a division of natural philosophy at least since Aristotle. However, it was in the early modern period that it emerged as a branch of science in its recognizably modern form, and this is due in large part to its nurturing within the embrace of experimental natural history. The Boylean project for the history of the air, with its support in the writings of Christopher Wren, Hooke, Richard Towneley, and the Oxford Philosophical Society, was almost certainly the first phase of the systematic accumulation of meteorological data that led to modern study of the weather. Two of Boyle's last natural philosophical publications, *Insalubrity and Salubrity of the Air* (1685) and *General History of the Air* (1692), with the Lockean endorsement of the latter's Baconian character,[65] are structured according to the method of experimental natural history. This, in turn, was the inspiration for the collecting and structuring of meteorological data up to the time of James Jurin and beyond.[66]

Conclusion

Experimental natural history was a widely practiced and, in some quarters, carefully theorized natural philosophical methodology at the forefront of scientific activity in the latter half of the seventeenth century. It is fair to say, however, that until the recent phase of careful scholarship on the natural historical works of Bacon over the last few decades, this form of

[62] Woodward (1695); Hooke (1705, 281–289); Buffon (1749–1789, 1:183–188).
[63] See, for example, Plot (1677, 1); Plot (1686).
[64] For an exemplar that cites Plot, see Hearne (1709). See also Jurin (1719).
[65] See Boyle (1999–2000, 10:303–349, 12:5–6). [66] See Jurin (1723).

natural history has too often been overlooked or, worse, conflated with classificatory natural history. More needs to be done to uncover, beyond the main figures who contributed to the theory of experimental natural history – Bacon, Boyle, and Hooke – other works and authors relevant to the subject. One likely candidate for future research is the eighteenth-century chemist and Baconian Peter Shaw, whose summary of the theory of experimental history in his 1733 edition of Bacon's *Works* reveals a subtle grasp of the theory and its application.[67]

In this chapter, we have attempted to disentangle some of the most important elements of this theory. We have shown that experimental natural history had within it a philosophy of experiment and a highly structured, exhaustive set of desiderata for the study of nature. Not all natural philosophers applied or even endorsed the method, yet scores of natural philosophers, savants, collectors, physicians, and travelers saw themselves as contributing to or promoting this architectonic project. The spirit of enthusiasm for the project is nicely captured in the comment made in March 1666 by one of Oldenburg's Parisian correspondents: "I talk everywhere about the fine plan which you [the Royal Society] have of making a natural history, in order to stir up the spirit of emulation among our learned men and to prompt them to imitate you."[68]

From experimental natural history we also can see new disciplines emerging; some rather short-lived, others with a lasting legacy. We can call these disciplines Baconian sciences, reinstating a term first introduced by Thomas Kuhn in a seminal paper.[69] Yet, Kuhn's meaning has to be self-consciously updated, to reflect the complexity and the lasting legacy of seventeenth-century experimental natural history.

[67] Bacon (1733, 2:567–568). [68] Oldenburg (1965–1986, 3:68).

[69] See Kuhn (1977b). Kuhn's Baconian sciences are defined, in sharp opposition to sciences properly speaking, as aggregates of practices, such as data collecting and fact gathering in the pre-paradigmatic stage. As we have seen in this chapter, although Baconianism does not provide a paradigm in the Kuhnian sense, it does provide a certain kind of unity of practice, sometimes even a common methodology, hence enabling practitioners to communicate and exchange results. Interestingly, Kuhn provides a good example of a scholar who did not appreciate the nature and complexity of experimental natural history. See Anstey (2014, 126–128).

13

Celestial Physics

PIETRO DANIEL OMODEO AND JONATHAN REGIER

Introduction

In Platonic philosophy, there is a fundamental dichotomy between the intelligible and the sensible. The intelligible forms, immaterial and incorruptible, underlie the order ingrained in our material world. They are imbued with ontological necessity, whereas what transpires in the sensible world around us is characterized by contingency. This dichotomy is still present in Aristotle, only it has, as Pierre Aubenque wrote, become *physical*.[1] Aristotle offers the heavens as a realm of orderly and necessary motions, opposed to the mutability and contingency of the sublunar region. The concentric and uniform motion of heavenly bodies plays an essential role in Aristotelian natural philosophy and, indeed, ontology. But Aristotle's writings provide no technical astronomy for predicting the actual motions of heavenly bodies. Soon enough, Greek astronomy would introduce geometrical tools contradicting Aristotle. The crowning achievement of Greek astronomy, Ptolemy's *Almagest*, proposed an accurate planetary theory using non-concentric circles and a device called the equant. The equant, an innovation of the first order, was an elegant way to make the planets change speed in their courses. While Ptolemy's techniques gave excellent results, they clearly deviated from Aristotelian requirements: Ptolemy had the planets revolving around the Earth with eccentric, non-uniform motion. In the Islamicate Middle Ages and Latin Renaissance, there were major attempts to reconcile Aristotle's physical explanations with Ptolemy's mathematical descriptions. Yet a universally accepted synthesis never emerged.

According to standard narratives about the Scientific Revolution, Copernican heliocentrism demanded new physical explanations, both of the motions of the planets and of the behavior of bodies on a moving

[1] Aubenque (2005, 314).

Earth. These were ultimately achieved with the advent of Newtonian mechanics, which merged mathematical astronomy with physics. It dissolved the Aristotelian dichotomy, extending the ontological purity of the celestial region to all matter or, vice versa, bringing the corruptibility of terrestrial existence into the heavens. All bodies, from a star to a speck of dust in a current of air, were now presumed to follow the same laws.

These traditional narratives draw a direct, continuous line from Nicolaus Copernicus to Isaac Newton. Alexandre Koyré described Copernican astronomy as the crucial first step in the transformation of the closed cosmos of Aristotle to the boundless universe of Newton.[2] Thomas Kuhn similarly framed early modern scientific developments as a "Copernican Revolution," which subsequently became his paradigm, of sorts, for scientific *revolutions* in the plural.[3] There is, however, as Koyré and Kuhn would both concede, an enormous gulf of knowledge and technique between Copernicus and the Newtonians; the connection is not as direct as it might seem. In fact, Copernicus was not always considered a watershed figure. Auguste Comte, the founder of positivism, thought that Copernicus did little but offer an ancient theory without removing the principle obstacle standing in the way of its acceptance – i.e., the Aristotelian account of terrestrial phenomena. True heliocentric astronomy required Galileo and the invention of what Comte called a "rational mechanics."[4] Ernst Mach, from the perspective of the history of mechanics, almost totally ignored Copernicus, stressing instead the achievements of Johannes Kepler and Galileo Galilei in celestial physics and dynamics.[5]

In this chapter, we will consider how astronomers and natural philosophers of the sixteenth and early seventeenth centuries sought to unify mathematical astronomy with physical and metaphysical causes. In doing so, we will explore several related themes, much debated in the period: the order of the celestial bodies and their nature, the relationship between celestial and terrestrial phenomena, the question of celestial animism or vitalism, and the status of the divine in celestial nature. While focusing on the figures of Copernicus and Kepler, we will also touch upon a variety of thinkers with diverse methods and interests.

Early Sixteenth-Century Physics and Astronomy

In Scholastic natural philosophy, the heavens operate by rotating celestial spheres that carry the planets and stars. Medieval natural philosophy, both

[2] Koyré (1957). Also see Koyré (1965).
[3] Kuhn (1959); Kuhn (1962). See Omodeo (2016a, 61–86).
[4] Comte (1835, 145–146).
[5] Mach (1988, 211–212).

Islamicate and Latinate, inquired into the nature of these celestial causes, which could be intelligences, angels, or souls. Mathematical astronomers, on the other hand, did not usually write about *how* the spheres turned, this being the province of philosophers and theologians, but there was wide consensus that they existed. This consensus carried on into the Renaissance.[6] Nevertheless, Ptolemaic astronomy relied on intersecting circular motions around many centers, which thus could not be straightforwardly attributed to solid spheres. Since the time of Ptolemy, some astronomers had argued that a system of partial, nested, eccentric spheres could reproduce Ptolemaic eccentrics and epicycles by way of non-intersecting three-dimensional bodies. Sixteenth-century university students typically learned such a system from Georg Peurbach's *Theoricae novae planetarum* (New theories of the planets) (1472), which systematized models developed by Islamicate astronomers and was widely adopted. Still, it preserved the philosophically problematic aspects of Ptolemaic astronomy: multiple centers and non-uniform motion produced by the equant. Nevertheless, the scheme could at least provide reassurance that Ptolemaic models might be reconciled to Aristotelian physics, at least in principle.

The inconsistency between predictive models and established natural philosophy periodically gave rise to dissension against Ptolemaic astronomy, and so against eccentric spheres. One episode featured the thirteenth-century Andalusian scholar Averroës (ibn Rushd), who insisted on the primacy of Aristotle's natural philosophy. Averroës identified the Aristotelian prime mover with the monotheistic God; separate intellects, he thought, animated each of Aristotle's celestial spheres.[7] In his commentaries on *Metaphysics* XII, Averroës asked astronomers to reject Ptolemaic models and ascribe planetary motions to concentric spheres instead, in accord with Aristotelian philosophy. Meanwhile, Averroës' contemporary, Alpetragius (al-Biṭrūgi), developed a complex theory of planetary motions guided by such principles.[8]

Early sixteenth-century celestial physics in Europe was marked by the convergent reception of Averroës and Alpetragius. Averroës' cosmopsychology was well established in universities by Copernicus's time – e.g., the philosopher Pietro Pomponazzi lectured on Averroës' *De substantia orbis* at Padua[9] – while a Latin translation of Alpetragius's *Kitāb fi'l-hay'a* was published in Venice under the title *Planetarum theorica physicis rationibus*

[6] Barker (2011). [7] Averroës (1986).
[8] For a brief overview of Islamicate critics of Ptolemy, see Evans (1998, 396–397); Morrison (2013, esp. 121–127).
[9] Pomponazzi (1966).

probata (Planetary theory demonstrated through physical proofs) in 1531. Paduan scholars took inspiration from their combined physico-mathematical program and created novel astronomical systems relying on homocentric spheres. The most prominent of these works were Giovanni Battista Amico's *De motibus corporum coelestium iuxta principia peripatetica, sine eccentricis et epicyclis* (On the motions of the celestial bodies according to peripatetic principles, without eccentrics and epicycles) (1537) and Girolamo Fracastoro's *Homocentrica sive de stellis* (Homocentrics, or On the stars) (1538).[10]

Copernicus can be situated within this tradition of astronomers attempting to harmonize astronomy with philosophical principles. The uniformity of celestial motions is fundamental to his physics and mathematical astronomy. While his commitment to uniformity may have been motivated by belief in the reality of celestial orbs, the clearest justification for uniformity given by Copernicus is metaphysical: irregularity could not be expected of these "objects constituted in the best order."[11] In any case, he did not think that his contemporaries' homocentric theories agreed with observations.[12] His *De revolutionibus orbium coelestium* (On the revolutions of the celestial spheres) (1543) deploys mathematical techniques in order to remove the more egregious irregularities attributed to the heavens by Ptolemy.[13] Copernicus's early readers recognized these ambitions and, in many cases, lauded his successes.

Copernicus, Mathematics, and Nature

In *De revolutionibus*, we see two features that are striking from the perspective of sixteenth-century natural philosophy. The first is an insistence that mathematical fittingness can say something conclusive about the structure of nature. The other is that Copernicus makes the celestial nature of the heavens a universal characteristic of matter.

[10] See Di Bono (1990).

[11] Copernicus (1543, 3^{r}); Copernicus (1978, 11). The consensus among historians of astronomy is that Copernicus had some commitment to the reality of celestial orbs. See Swerdlow (1973). For a still pertinent discussion of the controversy that surrounded this issue, see Jardine (1982).

[12] Copernicus (1543, iii^{v}); Copernicus (1978, 4).

[13] Copernicus used a minor epicycle to replace the equant. This technique is identical to one found in Ibn al-Shātir (1304–1375). In order to reproduce alleged variations in the rate of equinoctial precession, he used the Tūsī couple, which had been introduced by Nasīr al-Dīn al-Tūsī (1201–1274). Amico and Fracastoro had also both used the Tūsī couple in their homocentric systems.

Copernicus's mathematical models of uniform planetary motions have the shocking consequence that the Earth no longer stands at the center of the planetary order. But he emphasized the simplicity and interconnectedness of his heliocentric astronomy. With a mobile Earth, astronomers could calculate the size of planetary orbs, using the Earth–Sun distance as a yardstick. All the planetary distances could then be expressed as a series of interrelated ratios that he calls the world's "symmetry." In addition to symmetry, the heliocentric arrangement yielded a "harmonious linkage" (*nexus harmoniae*) between planetary distances and their periods: the longer the period of revolution, the longer a planet's path around the Sun.[14] This correspondence between period and distance was a well-accepted principle in geocentric astronomy, yet from a modeling point of view, geocentric astronomers could order the planets however they liked.[15] By contrast, in *De revolutionibus*, the period–distance relationship proves necessary. The new order, Copernicus argues, also explains planetary phenomena that otherwise seem unrelated to one another, like the peculiarities of retrogradation (especially the relationship between retrogradation and a planet's opposition or conjunction with the Sun).[16] Many details in Ptolemaic theory work without any underlying reason; the astronomer just has to follow the rules. Copernicus saw these details as clues leading to the Earth's motion as a common cause. What we find in *De revolutionibus* is an astronomer showing how mathematics, or *mixed* mathematics, describes the structure of nature over and above the conclusions of natural philosophy or "physics" (as it could also be known). To put it another way, Copernicus yokes physics to astronomical demonstration, which he values according to a certain conception of elegance or fittingness.

The geometrical order that Copernicus celebrated put him in contradiction with Aristotelian physics when it came to explaining terrestrial phenomena. Aristotle's robust theory of sublunary elements and their motions both depended upon and supported the centrality and immobility of the Earth – in particular, Aristotle explained that the four Empedoclean elements (earth, water, air, and fire) naturally move rectilinearly toward or away from the unique and immobile center of the world. Copernicus also had to deal with the basics of experience. In the *Almagest*, Ptolemy had described some of the

[14] Copernicus (1543, 9^{v}–10^{r}); Copernicus (1978, 22). For Copernicus's use of *symmetria*, see Hon and Goldstein (2008, 157–163).

[15] Aristotle uses this principle to establish planetary distances in *De caelo* II.10 (291a29–291b10) (Aristotle 1984, 1:480).

[16] For a list of technical points that Copernicus saw as confirming his astronomy, see Swerdlow (2004, 88–90).

phenomena that would presumably follow from the diurnal rotation of the Earth: for instance, a roaring westward wind, caused by the Earth turning at high speed beneath the air.[17] The obvious lack of such phenomena spoke against Copernicus's view. In addition, the absence of stellar parallax meant that the Earth–Sun distance was negligible compared to the distance from the Earth to the fixed stars. His astronomy implied that the universe was unfathomably large.

In response, Copernicus embraced this new cosmic vastness, although he left it to philosophers to decide whether the universe was truly infinite. Copernicus offered some sketchy physical arguments to convince his readers that terrestrial motion was actually possible. His answer for why we do not perceive the Earth's motions is that we participate in them. He invoked the now-famous analogy with a moving boat in which the passenger cannot immediately tell if the shore is slipping away from the boat or the boat from the shore.[18] Copernicus also emphasized a point of similarity between the terrestrial and celestial bodies: they are spherical. Of course, for Aristotle, the perfectly smooth heavenly spheres were much more exactly spherical, but Copernicus subverts this position by making the Earth the standard of spherical perfection – calling it an "absolute" sphere. He asks readers to consider the Earth as a geometrical whole; although our landscape is full of elevation changes, these discrepancies are negligible compared to the overall size of the globe.[19] Ancient mathematicians had established the Earth's almost perfect sphericity, but Copernicus draws on the latest geographical discoveries by Christopher Columbus and Amerigo Vespucci for evidence that the elements of earth and water share the same spherical surface (something that Ptolemy had argued for in his *Geography*).[20] Given that the celestial bodies are spheres, and given that they undergo circular revolutions, we should not then deny the Earth its own revolutions. This might not demonstrate any kind of physical necessity, but it argues that terrestrial motion is not physically *impossible*.

As for how this is physically *possible* – how the Earth can maintain its own center, despite its varied motions – Copernicus is not especially clear. He says that the matter of a planet holds together in a sphere through some natural desire (*appetentia*), implanted by God, to unite with its own kind.[21] In turn, the Earth's motions are shared with all of its constituent bodies. Note that this account obviates any dependence on a fixed center of the world. Copernicus

[17] Ptolemy (1998, 45). [18] Copernicus (1543, 6^{r}); Copernicus (1978, 16).
[19] Copernicus (1543, 1^{r-v}); Copernicus (1978, 8–9). [20] See Vogel (2006, 480).
[21] Copernicus (1543, 7^{r}); Copernicus (1978, 18). See Hooykaas (1987); Knox (2005).

also reframes all rectilinear motion as inherently violent. There is only one natural motion: circular revolution. Hence, Copernican physics "celestializes" the terrestrial, such that the motion of celestial bodies becomes the shared motion of all things.

Copernicus's appeal to an inherent *appetentia*, however vague, seems to have been inspiring. Early defenders of terrestrial motion like Celio Calcagnini, around 1518, and later Giordano Bruno, William Gilbert, and Kepler saw terrestrial motion as dependent, fully or partially, on celestial animation, the position that some or all celestial bodies were equipped with souls. Kepler observed that, "Copernicus preferred to think that the Earth and all terrestrial bodies (even those cast away from the Earth) are informed by one and the same motive soul [*una et eadem anima motrice informari*], which, while rotating the Earth, also rotates those particles cast away from it."[22]

Renaissance Vitalism and the Fluid Heavens

From Greek philosophy through to its medieval Arabic and Latin commentators, the celestial bodies were thought to be alive or moved by separate intelligences. In Scholastic philosophy, the latter option was much preferred. The idea of stars and planets endowed with souls struck too pagan a chord and conflicted with widely held views on God's rule over nature.[23] In the sixteenth century, though, life came flowing back into the cosmos. Indeed, innovative cosmologies of the period were supported by an appeal to vital causes in the heavens. For example, the same Fracastoro whose *Homocentrica* was a counterpoint to Copernicus participated in this vitalist turn. He had debated the causes of celestial motions as early as 1531, and his readers asked for a more thorough investigation of the causes. Among them, the Venetian intellectual Gasparo Contarini invited Fracastoro to revive the doctrine of *Metaphysics* XII in order to provide a solid foundation on which to ground celestial motions in accordance with Aristotelian commentators such as Averroës and Alexander of Aphrodisias.[24] In the event, Fracastoro adopted a different physics entirely. In *Fracastorius, sive de anima, dialogus* (Fracastoro's dialogue, or On the soul) (posthumously published in 1555), he regarded planets as animal-like bodies whose souls are akin to the world-soul (*anima mundi*), which infuses life and motion throughout all parts of the universe.

[22] Calcagnini (1544); Kepler (1937, 3:28); Kepler (1992, 58–59).
[23] See Dales (1980); Grant (1994, 469–487). [24] Contarini (1571, 238–252, esp. 240).

Because planets derive their powers from the world-soul, their vital and intellectual faculties are rooted in nature and do not presuppose separate intelligences, *pace* Averroës.[25]

Fracastoro's natural philosophy owes a complex debt to Marsilio Ficino.[26] A principal textual source behind the sixteenth-century animation of the heavens was the reintroduction of Platonic and Neoplatonic philosophy through Ficino's translations and commentaries.[27] His *De vita libri tres* (Three books on life) (1489) – particularly its third book, *De vita coelitus comparanda* (On obtaining life from the heavens) – proved extremely influential on an important cross-section of natural philosophers, making the twin concepts of cosmos-infusing *anima mundi* and *spiritus mundi* philosophically attractive. The wider point is that explanations of natural phenomena, including those in the celestial realm, began to appeal to various kinds of non-Aristotelian natural philosophy, as well as non-literal forms of Aristotelianism. Most significantly, whereas in the first half of the sixteenth century, there was a more or less uniform acceptance of corporeal celestial spheres,[28] during the second half, a rival view achieved wide acceptance – that of an open, fluid heavens through which the celestial bodies move freely.

The first known measurement-based refutation of solid spheres came in 1557 from Jean Pena, who concluded in a Stoic vein that the entire universe must be filled with the same air that envelops us.[29] Pena's work influenced the astronomer Christoph Rothmann, who in turn played a role in the development of Tycho Brahe's conclusions about the composition of the heavens.[30] Brahe's renowned treatise, *De mundi aetherei recentioribus phaenomenis* (On the more recent phenomena in the ethereal world) (1588), was the most influential affirmation of heavenly fluidity. In it, he reviewed the debates and publications on the comet of 1577–1578, established its parallax and distance, and concluded that its location was above the Moon, in the space once deemed to be occupied by incorruptible material spheres. Since the comet freely moved there, the superlunary region had to be fluid. With the absence of solid spheres, however, it became urgent to establish the cause of planetary motion.[31] Brahe resorted to a "science infused by God" in the celestial bodies, governing their free motion through space.

[25] Fracastoro (1574, 149^{v}–150^{r}). [26] See Pennuto (2008, 7, 12).
[27] See, for example, Hankins (1999).
[28] On the material characteristics of solid spheres in the medieval period, see Grant (1987a, 172–173).
[29] Barker (2008, 273–274).
[30] Rothmann (2014). Also, see Goldstein and Barker (1995); Mosley (2007, 77–78).
[31] Granada (2010).

Other scholars who accepted heavenly fluidity or vacuum also ascribed souls to the planets capable of motivating their motions. Among them, the Neoplatonic thinker Francesco Patrizi revived the Stoic image of the planets moving themselves through the fluid heavens like fish in water and birds in the air.[32] In a much different philosophical framework, the Copernican heretic Bruno defended a similar view of the planets moving through an infinitely expansive aether.[33] Bruno's framework was marked by an idiosyncratic kind of atomism and by a forcefully argued position that God's essence implied a universe without boundaries or any privileged center. In his many philosophical works, Bruno asserted a boundless, living universe occupied by countless *synodi ex mundis*; that is, heliocentric planetary systems encircling each star.[34] For Bruno, the celestial bodies are big animals, endowed with sensible and rational souls governing their organic functions and movements. Specifically, heavenly bodies move through space in order to make the exchange of life and warmth between fiery suns and cold earths possible.[35] The English physician and philosopher William Gilbert, who maintained interplanetary space to be a vacuum – a very rare opinion – would later hold a similar view about the Earth's diurnal revolution.[36] Kepler, who believed space to be filled by a tenuous aether, would also consider the celestial bodies alive (although not intellective), thereby accounting for the production of celestial forces on which to base his new astronomy. He would also speculate about the nature and activity of the the Sun's soul as a kind of world soul, generating with its warmth new stars and comets in the celestial reaches.[37]

Kepler's Natural Theology and Celestial Physics

Kepler, Imperial Mathematician to the Holy Roman Emperors Rudolf II and Matthias, justly occupies a central place in the history of celestial physics. He was in many ways typical of the reception of Copernicus. He connected the new astronomy to the divine and sought to create for it a plausible natural philosophy. But he was in other ways singular. He argued in detail for a method of discovery and confirmation that intertwined mathematical and physical considerations; wherein geometrically equivalent astronomical models could be compared via their physical consequences.[38] His physics made no distinction between celestial and terrestrial – we can understand celestial matter and forces by examining objects at hand. His physics,

[32] Rosen (1984). [33] Grant (1981, 188–189). [34] Granada (2007).
[35] Bruno (1962, 81). See Gatti (1999, 121). [36] Gilbert (1958, 224).
[37] For Kepler's vitalism, see Boner (2013). [38] See Jardine (1984); Martens (2000).

informed by his mastery of optics and mechanics, was also highly mathematized, emphasizing the quantification of bodies, distances, and spatial orientation. For example, he believed that the force of the Sun was related to its mass and density.[39] Consequently, Kepler pushed the reform of astronomy much further than any of his peers, basing it on physical causes and granting it authority to speculate about matter and force.

Kepler's work was rooted in a specific natural-theological context. The Reformation philosopher Philipp Melanchthon, an architect of Lutheran higher education, taught his pupils that the celestial motions reveal God's wisdom and exert a providential influence on the sublunary realm. At the University of Wittenberg, where he was professor, Melanchthon promoted natural philosophy, the mathematical arts, and medicine, as so many ways of witnessing God's presence and wisdom.[40] Melanchthon used his institutional authority to promote the careers of the mathematicians Georg Joachim Rheticus and Erasmus Reinhold. Rheticus became the disciple of Copernicus and authored the *Narratio prima* (First account) (1540), the first published exposition of Copernican astronomy. It was also Rheticus who spearheaded the publication of Copernicus's *De revolutionibus*. Reinhold reintegrated Copernican innovations within a geocentric framework and used Copernicus's models to calculate his *Prutenic Tables*, soon the most widely used astronomical tables in Europe. Reinhold's reception of Copernicus – taking the mathematics while passing on the physics – typifies what the historian Robert Westman named "the Wittenberg interpretation of Copernicus."[41] Unsurprisingly, Melanchthon's own reception of Copernicus was complex. At first, he violently rejected the physics on Ptolemaic, Aristotelian, and, most importantly, biblical grounds. But he listened to his mathematician colleagues enough to eventually appreciate the technical innovations of Copernican astronomy, as well as what he took to be its eschatological implications. Copernicus had asserted in Book III of *De revolutionibus* that the eccentricity of the Sun was steadily diminishing. Melanchthon seized on this, seeing it as a sign of the approaching end times and return of Christ.[42]

Kepler, educated at the University of Tübingen, was a product of the Melanchthonian program. At Tübingen, where he was studying for the Lutheran priesthood, he had the good fortune to learn astronomy from the mathematics professor Michael Maestlin, an eminent astronomer and

[39] Regier (2014). [40] Kusukawa (1995). [41] Westman (1975); Westman (2011, 150–164).
[42] Lerner (2006, 442–444).

one of Europe's few convinced Copernicans. Kepler firmly believed that our world was a unique creation bearing everywhere the stamps of the divine mind. The cosmos had to be ideally structured and singular. He also believed that the ideas behind the world's creation – the blueprint, as it were – had to be within the bounds of human comprehension. Kepler thus criticized the thesis of an infinite universe – particularly in *De stella nova* (On the new star) (1606), *Dissertatio cum Nuncio sidereo* (Conversation with the sidereal messenger) (1610) and *Epitome astronomiae Copernicanae* (Summary of Copernican astronomy) (1618–1621) – not just because he saw it as an expression of Epicurean impiety.[43] It threatened to make the structure of our planetary system – the best possible structure – redundant, unnecessary, even unintelligible.

The goal of Kepler's first published book, *Mysterium cosmographicum* (Cosmographical mystery) (1596), was to justify Copernican astronomy, taking as seriously as possible Copernicus's promise that his astronomy brought us closer to the divinely conceived structure of the world. Since the Earth's movement made it possible to calculate the sizes of the planets' orbits, Kepler sought to explain why they are endowed with certain sizes and not others. He professed to demonstrate the reason why through the Platonic solids; i.e., the five regular polyhedra. These solids could be interposed between the planetary paths to arrive at a system of proportions yielding (approximately) the Copernican distances. Part of the solution's appeal was the use it made of mathematical objects charged with value. The solids have a world-building role in Plato's *Timaeus*, where they serve as the forms of the elements, and they are also the subject of the last book of Euclid's *Elements*.[44] Kepler could offer his discovery as an unearthing of lost ancient wisdom, even suggesting that Pythagoreans had once known the cosmic function of the Platonic solids but had kept their secret from outsiders.[45] Kepler's archetypical reflections culminated in *Harmonice mundi* (Harmony of the world) (1619). Here, he combined several metaphysical elements into a single coherent world system: the polyhedral hypothesis of the *Mysterium*, which explained the planets' distances to the Sun, was combined with Kepler's own theory of harmony to explain their motion. All of this was fitted to a long meditation, heavily indebted to Proclus, on the geometrical nature of

[43] Boner (2007).

[44] Kepler described both the *Timaeus* and *Elements* as "Pythagorean" works. In fact, he saw Euclid transmitting in his *Optics* "a pure unadulterated Copernican astronomy" (Kepler 1937, 3:284–287; Kepler 2000, 342–345).

[45] Kepler (1937, 6:81); Kepler (1997, 115).

souls, their relationship with the divine, and their instinctive mathematical knowledge.[46]

Kepler's theological-harmonic speculation had important predecessors in fifteenth- and sixteenth-century scholars who had embraced the Platonic idea of cosmic harmony.[47] Rheticus, Copernicus's disciple, had written that heavenly symmetry and harmonious linkage are best understood through an immediate apprehension of the mind rather than by language: "not so much by words as by the perfect and absolute ideas, if I may use the term, of these most delightful objects."[48] Girolamo Cardano, one of the most original philosophical and mathematical intellects of the sixteenth century, had formulated a program belonging to a geometrical style of thought similar to Kepler's. In his *Encomium geometriae* (Praise of geometry) (1535), he affirmed, without going into detail, that one could reconstruct the secret proportions of creation assuming that God, the greatest geometer, had constructed the cosmos according to proportions derived from the internal structure of geometrical figures. According to Cardano, one could unveil the ratio of the distances of the seven planets by following such a divinely inspired construction method.[49]

Kepler's quest to unveil God's construction of the world also led him to seek the active causes by which the heavenly motions are produced, principally the interaction between Sun and planets. This produced Kepler's now most famous treatise, significantly titled *Astronomia nova ΑΙΤΙΟΛΟΓΗΤΟΣ seu physica coelestis* (New astronomy from the causes, or celestial physics) (1609).[50] *Astronomia nova* rested on highly accurate observations made by Brahe together with his team of assistants. Framing his work as a narrative of struggle and discovery, Kepler demonstrated that planetary orbits are ellipses with the Sun occupying one of the foci (later known as Kepler's "first law"), that from the Sun emanates a moving force, and that, consequently, planetary speed changes in a calculable way as the planet's distance from the Sun increases or decreases (according to what has later become known as the "second" or "area law"). Kepler expected his astronomical models to describe physical causes – real bodies and real forces. For instance, the area law worked, according to Kepler, because the areas in question represented infinitely many moments of impulsion radiating from the Sun.[51] For his

[46] For Kepler's harmonies, see Walker (1967); Stephenson (1994).
[47] This along a line traced from Ficino's reflections on the *Timaeus*. See Prins (2015, 80–118).
[48] Kepler (1937, 1:104); Rosen (1939, 145). [49] Cardano (1966, 4:445b).
[50] For a thorough discussion of Kepler's astronomy and its physical grounding, see Stephenson (1987); Voelkel (2001); Davis (2003).
[51] Aiton (1975a); Aiton (1975b).

conception of the solar force, he drew inspiration from Gilbert's *De magnete* (On the magnet) (1600) and concluded that the Sun was a magnetic body. Later, in the *Epitome astronomiae Copernicanae*, he enlarged his theory of planetary motion to include magnetic filaments in the planetary bodies. Kepler's project to explain celestial motion by natural forces did not stand in contradiction with his thoughts on celestial souls. He felt that his physical astronomy made *intellective* souls unnecessary in the heavens. But he still believed animal souls were needed to account for generation and active forces in nature. Souls remained, for Kepler, the motors of nature.[52]

Alternative Programs

Kepler's celestial physics left many discontented, including Brahe's pupil Christianus Severinus Longomontanus, professor at Copenhagen, who attacked Kepler's novel approach because it infringed on accepted disciplinary distinctions between metaphysics, physics, and mathematical astronomy. He accused Kepler of impiety since the latter reduced celestial *meta*-physics to mere celestial physics.[53] Longomontanus's *Astronomia Danica* (Danish astronomy) (1622) can be considered the apex of the geometrical tradition in astronomy, which modeled planetary motions by means of epicycles in the geo-heliocentric framework that Tycho Brahe had claimed as his own invention and vigorously promoted. In this framework, the Earth remains immobile; the Sun circles the Earth, and the planets circle the Sun. One of the obvious advantages of such a combination concerned sublunar physics, since it secured the central and immobile position of the terrestrial region. Moreover, it maintained the key postulate of Scholastic celestial physics: that is, perfect celestial bodies follow laws that differ from those governing terrestrial matter.

Likewise, Daniel Cramer, a professor of the Stettin Gymnasium, had discussed these issues with Brahe and suggested a manner in which geo-heliocentric, fluid-heaven astronomy could and should be reconciled with Aristotle's *Metaphysics* XII. Cramer dedicated to Brahe an *Isagoge in Metaphysicam Aristotelis* (Introduction to Aristotle's *Metaphysics*) (1594 and 1601), in which he discussed the causation of celestial motions. According to Cramer (who was followed by several Baltic astronomers), one could resort to the separate intelligences of the Averroist tradition and apply them

[52] For the role of solar soul in Kepler's late astronomy, see the *Epitome* (Kepler 1937, 7:298–299).

[53] Longomontanus to Eichstaedt (Copenhagen, June 6, 1638), in Eichstadius (1644, 148, 151).

directly to the bodies of the planets, instead of to non-existing celestial spheres.[54]

Kepler was not unprepared for such criticism, arguing in response that no causal account could explain how the massive body of the Sun might circle the Earth.[55] As for religious concerns, he stressed that Copernican astronomy was better suited to Christian faith. The uniqueness of the source of heavenly motions, firmly located at the center of the world in the Sun, mirrors the uniqueness of its creator; by contrast, the separate celestial intelligences, which late-Scholastic Tychonians (like Cramer) embraced, seemed to revive the polytheism implicit in Aristotle.[56] Nevertheless, Kepler's objections discouraged neither his Protestant opponents nor Jesuit neo-Thomists from working within a geo-heliocentric paradigm, as witnessed in the Italian reception of Brahe by Christoph Clavius and Giovanbattista Riccioli in the seventeenth century.[57]

Mechanical objections to Kepler's physics were of a different tenor. In the fourth day of the *Dialogo sopra i due massimi sistemi del mondo* (Dialogue concerning the two chief world systems) (1632), Galileo mocked Kepler's explanation of sea tides based on the distant action of the Moon upon the waters. Instead, he developed the thesis of his Pisa professor, Andrea Cesalpino, that the tides were produced by the combination of the Earth's diurnal and annual motions. In fact, Galileo treated this explanation of the tides as the strongest argument in favor of Copernicanism. Giovanni Pico della Mirandola's criticism of astrology and occult virtues loomed large in Cesalpino's and Galileo's rejection of lunar influence.[58] Anti-astrological skepticism cast into doubt the very foundations of Kepler's celestial physics, even if Kepler had largely followed Pico's restrained view of celestial causes.[59]

More generally, the rejection of action at a distance and final causes became two tenets of seventeenth-century mechanist philosophies. Their alternative programs envisaged a physics resting on a small set of laws governing bodily motions and interactions *everywhere*. Pierre Gassendi, heavily involved in astronomy throughout his career, drew from Kepler's theory of a magnetic Earth, but while Kepler had been adamantly anti-Epicurean, Gassendi reimagined the magnetic filaments as atoms causing our diurnal rotation.[60] René

[54] Omodeo (2016b). [55] Kepler (1992, 169–170). [56] Omodeo (2015).
[57] Lerner (1995).
[58] Omodeo (2017).
[59] For Kepler and Pico, see Rabin (1997). For Kepler's astrology, see Field (1984).
[60] Sakamoto (2009).

Descartes similarly rewrote physics according to a new corpuscular conception of the world. In his *Le monde* (The world) (written before 1633 and first published in 1664) and in the *Principia philosophiae* (Principles of philosophy) (1644), he defended the vision of an indefinitely large universe bearing significant resemblance to Bruno's as to its overall structure, but not as to its principles. For Descartes, celestial bodies do not autonomously move owing to an inner impulse, as Renaissance vitalists had claimed. Rather, they are transported by fluid vortices of subtle ethereal matter. Moreover, he took cosmological homogeneity much further than his Renaissance predecessors; for him, the corruptibility of the heavens implied that stars could become comets and that comets could then transform into planets.[61] These trends reached their climax with the work of Newton, whose *Philosophiae naturalis principia mathematica* (Mathematical principles of natural philosophy) (1687) synthesized celestial and terrestrial physics into a unified system of gravitational interactions between bodies. Notably, Newton's gravitation retained the flavor of Kepler's attraction at a distance, even if Newton avoided explicit commitment to any causal explanation of gravity.

Concluding Remarks

We would stress that the achievements of sixteenth- and early seventeenth-century cosmology were ultimately due to the remarkable liberties available to astronomers. Thanks to humanism and printing, many causal frameworks were available. Political and social contexts, which fall outside the purview of this chapter, also played an undeniable role. It has long been understood that opportunities of court patronage gave figures like Brahe, Kepler, and Galileo, distinct intellectual freedom over most university professors. A patron like Rudolf II, the Holy Roman Emperor, expected novelty; his Imperial Mathematician, Kepler, was ready to provide it. Of course, intellectual freedom is never total. It is always contingent on local powers, as the fates of Bruno and Galileo remind us. If the celestial revolution was lasting, it was for the following reason: our vision of the heavens underwent such a variety of transformations during the sixteenth and seventeenth centuries, and in such a variety of contexts, that by the end, short of almost inconceivable tyranny or social collapse, it would have been impossible to return to the cosmos taught at the close of the fifteenth century. Here is perhaps

[61] Aiton (1972).

the sense in which we might best understand this intellectual revolution: at a certain point, it became impossible to reverse direction on so many byways and avenues that may have, over the short term, once allowed two-way traffic.[62]

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14

Applying Mathematics to Nature

MAARTEN VAN DYCK

Introduction: Mathematization?

> Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth.[1]

Galileo's famous quote is often taken to express one of the central characteristics of the Scientific Revolution: the ideal of a thoroughgoing mathematization of (the study of) nature. Yet it is easy to misconstrue the meaning of Galileo's metaphor by ignoring the specific context in which it was introduced. Galileo only characterized mathematics as a language to mock the Aristotelians' presumed dependence on human books as the ultimate authority – if there is an authoritative book that is to be read, it can only be nature itself, and its language cannot be of human origin.[2] One must also reconstruct the specifically mathematical background that made it appealing for a thinker like Galileo to invoke these striking images in this polemical context before reading strong metaphysical commitments into the metaphor.[3]

Stories about the Scientific Revolution usually focus on how ideas about physics and the natural world were profoundly modified in the early modern period, but they often silently assume an ahistorical notion of mathematics that hides the way in which the category of mathematics itself was also being reshaped in the process. There are different, though not unrelated ways in

[1] Galilei (1957, 237–238).
[2] See Biagioli (2003) for an analysis of the polemical background to Galileo's use of the metaphor. See also Palmerino (2016).
[3] See Hatfield (1990).

which the historicity of mathematics can be taken into account. One important line of corrections to earlier stories has involved paying close attention to the constraints imposed by the use of particular mathematical techniques and norms that are no longer visible from the perspective of present-day mathematics.[4] Another question concerns the notion of application itself: what did it mean to "apply" mathematics to the empirical world? This chapter will explore a way to understand the issue of application from an early modern mathematical perspective, and will use this analysis to offer a fresh perspective on the idea of the mathematization of physics.[5]

Unpacking the notion of application implies that we should try to understand how one learned to "recognize" Galileo's circles and triangles in the empirical world. Many historical analyses have drawn attention to the importance of the Aristotelian notion of subalternate or middle sciences.[6] These sciences were supposed to result from a two-way operation. First, pure mathematics is established by abstracting away all natural attributes from physical substances, so that only their quantitative attributes remain. Second, mathematical truths ascertained in this abstract setting can be predicated of natural phenomena by superadding specific physical attributes to them, so that, e.g., in the case of optics, we are no longer dealing with straight lines, but with straight lines in light. This results in the following picture of application. Circles and triangles are typically seen as the abstracted shapes of physical objects; mathematical truths established about them can be applied back to the empirical world if we know on natural philosophical grounds that specific phenomena exhibit some mathematical attributes. E.g., if we know on natural philosophical grounds that light moves in straight lines, we can use mathematical knowledge to demonstrate properties of optical phenomena.

Rather than start from this philosophically motivated picture, the second section of this chapter will sketch how sixteenth-century mathematicians approached the mathematization of often very mundane empirical situations through a set of concrete practices. Focusing on application as a practical

[4] See the chapter by Guicciardini in this volume (ch. 15).

[5] Roux (2010) warns against monolithic views on a development that actually had many forms. This chapter will identify one particularly important form, without the presumption that this would exhaust the field of options. Gorham, et al. (2016) is a recent volume that treats the issue of mathematization from a variety of angles.

[6] Machamer (1978) and Lennox (1986) were influential in linking Galileo's work to this notion. Laird (1983; 1987) analyzes Scholastic discussions. Distelzweig (2013) offers a concise treatment of Aristotle's own treatment. See also Mueller (1990) for different interpretations of Aristotelian abstraction.

issue will bring two related aspects to light: the relation between the empirical world and the mathematical domain was primarily mediated by an operation of construction rather than abstraction; and since this has important implications for the representational role of mathematical diagrams, it will also allow us to better see how mathematicians could hope to characterize an open-ended list of empirical phenomena in geometrical terms.

Galileo's metaphor invites a further question. Reading a book involves more than recognizing its characters; it also requires identifying narrative structures that allow the reader to make sense of the words and sentences formed. But what was the appropriate reading strategy for the book of nature – to what genre did it belong? As we will see, the mathematization of the study of nature did not just depend on the replacement of one language with another, it was also (and maybe primarily) a shift in reading strategy – one that focused on problem-solving, as was suggested by the practice of mathematics. The third section will try to gauge the impact that an explicitly mathematical way of approaching the empirical world could have on philosophical ideals and goals.

The following section will deliberately avoid connecting mathematical practice with philosophical debates of the period. This is in the first place a methodological choice to better bring out the specific nature of the practical operations that were central in applying mathematics to empirical phenomena, and that often get obscured by philosophical preoccupations. Both the thesis that Platonic metaphysics was the key to understanding Galileo's mathematization of nature and the thesis that this grew naturally out of Aristotelian developments have been defended with vigor in historical scholarship.[7] The use of these philosophical categories to frame the question of mathematization tends to divert attention from the crucial focus of mathematical practice itself: problem-solving through construction. There were other sixteenth-century philosophical developments that were closer in spirit to this practice, such as the Ramist movement and the rediscovery of Proclus's philosophy of mathematics.[8] These reflections on what it meant to engage in mathematical reasoning were definitely an important part of the background against which sense could be made of the idea of extending mathematics beyond its traditional domain to topics that had traditionally belonged to natural philosophy; a background that also included

[7] Koyré (1978) is the classic reference for the Platonic interpretation. Wallace (1984) has been an influential plea for the Aristotelian reading.

[8] For Ramus, see Goulding (2010); Pantin (2019). For Proclus, see De Pace (1993); Claessens (2009).

reconfigurations of elements from Platonic and Aristotelian philosophy. But it is important to lay bare the aspects of the mathematical practice itself that allowed such extension and that gave a particular direction to the resulting options for reconceiving natural philosophy. This is the angle from which the topic will be analyzed here.

The Practice of Mathematics in the Sixteenth Century

Let us begin with some triangles and circles. In 1571, the English mathematician Thomas Digges published *Pantometria*, a text on practical geometry written by his father, Leonard, before his death in 1559. It contained an entirely traditional treatment of the measurement of lengths, surfaces, and volumes. Consider the twenty-third chapter of the first book, which explains how to determine an unknown distance with the use of a geometrical square. What is the distance between locations *A* and *B* (in Figure 14.1)? The surveyor first uses his geometrical square (seen in operation at *C*) to construct a right angle upon the line of sight *AB*, and then picks out an arbitrary third location *C* lying somewhere along the perpendicular line. By directly measuring the distance between *A* and *C* and the angle *ACB*, the distance *AB* (and *CB*) can be readily determined.

The first thing to be noticed about this procedure is that the triangle *ABC* and the circular arc that measures the angle are not simply "abstracted" from

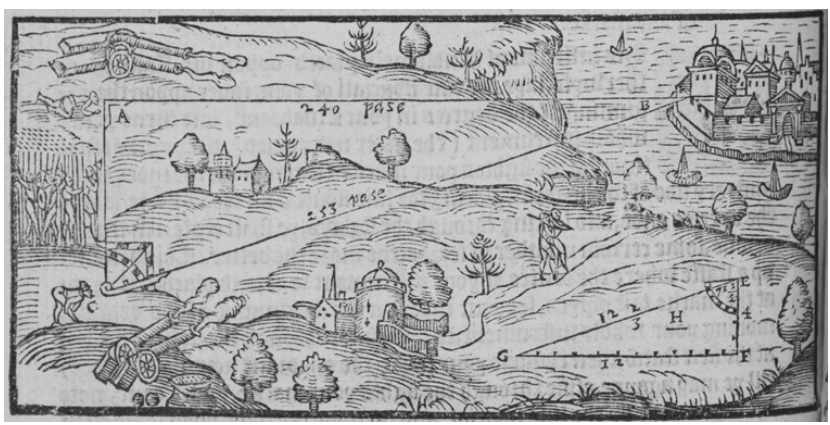

Figure 14.1. Determining an unknown distance
Source: Digges and Digges (1571, ch. 23). Courtesy History of Science Collections, University of Oklahoma Libraries

the situation. They cut across all empirically given shapes and boundaries in the landscape. Instead, the triangle is actively constructed by the instrumentally mediated operations of the surveyor, who is pictured in different poses that highlight his active engagement. In fact, the second triangle in the lower-right corner of the figure is one that the surveyor is supposed to materially construct on a piece of paper using ruler and compass. By inspecting this scale drawing, he can superimpose the geometrically established relationships on the landscape.

So what is the triangle in the figure doing if it is not representing any concrete shape? It solves a practical problem by showing how different measurable quantities are systematically related to each other. If you know any three quantities through direct measurement, you can immediately determine a fourth, unknown quantity. One can think of the triangle as geometrically encoding this relation; constructing the geometrical figure is a way of manipulating the accessible information. Seen from this perspective, "applying" mathematics to empirical situations is a matter of constructing geometrical diagrams to solve problems.

Digges's triangle, of course, provides only an elementary example. Sixteenth-century mathematicians spent considerable time and energy on developing mathematical instruments that could perform more complicated tasks, such as sundials (often of amazingly intricate design), quadrants, and astrolabes.[9] Most of these instruments can be understood as operating along similar lines. They gave their user the means both to effect a direct measurement (e.g., of the Sun's height above the horizon) and to extract more information from its result (such as the time of the day). The background knowledge required for this latter step was engraved on the instrument and often required further manipulations by its user, who moved parts that helped translate the inputted measurement into the output value. Circles and triangles were only a part of the repertoire of figures engraved on the instruments, which encoded, e.g., the Sun's altitude at each time of the day for all different dates of the year at a given latitude.

The geometrical engravings and moving parts of the more complicated instruments played a role very closely related to that of what was known as a *theorica*.[10] The term originated in astronomy, where a *theorica* was a geometrical model that allowed one to track the observed motion of a planet (which in turn could be embodied in a material instrument called

[9] See Bennett (2003) for an important analysis of the function of mathematical instruments.

[10] See Bennett (2003, 142–143); Johnston (2004).

an *equatorium*), but it was also applied in other fields that used geometrical constructions to determine relations between measurable quantities. Their status as models can be called "instrumental" in a more substantial sense than the one often intended in traditional narratives concerning the history of astronomy. They usually were not supposed to be straightforward "realistic" depictions of how things are, but neither were they to be judged merely by their predictive successes. Their predictive role could have been taken over by purely arithmetical or tabular methods, but their geometrical character was crucial since it allowed further conceptual and practical exploration of systematic dependencies. To put it differently, geometrical constructions were essential tools for both thinking about and actually effecting new measurements. They allowed users to immediately see the relations between a wide range of possible observations (e.g., between latitudes, dates, and times), and this spurred mathematicians to develop ever new ways of representing some of these relationships and embody them in new material instruments.[11]

There was an important continuity between "practical" and "pure" mathematics. This is illustrated in the treatise on Platonic solids that Thomas Digges appended to the edition of his father's text. This exercise in pure mathematics can again be understood as an exploration of systematic dependencies, explicitly intended to solve geometrical problems. Digges's Euclidean-style geometry was similarly grounded in constructive activity, even if not necessarily carried out materially, but typically supposed to take place in the imagination (based on "postulated" constructions of straight line and circles).[12] As Digges's preface made clear, the main difference he saw between his own pure and his father's applied mathematics was merely the goal to which the exploration of dependencies was put – in the one case the satisfaction of pure curiosity, in the other the achievement of practical ends.[13]

Given this continuity, Euclid's *Elements* could be seen (and was often explicitly presented) as a toolbox for practical mathematics. The attempt to extend problem-solving techniques to a field of practical operations was often described as the search for a "reduction to art."[14] Euclidean geometry itself can be considered a successful exemplar of this ideal. By identifying a few

[11] See Bennett (2012); Kremer (2016) for examples.
[12] To avoid possible misunderstanding: this claim does not rule out mathematical realism, as I am describing the kind of cognitive activity underlying mathematical thinking, not the ontological status of presumed mathematical entities. On the central place of construction in early modern geometry, see the classic study, Bos (2001).
[13] See Johnston (2006) for further analysis.
[14] See Vérin (2008) for an essential introduction.

elementary operations (drawing a straight line and a circle), and investigating their mutual relationships, Greek mathematicians had been able to show how any spatial measurement problem could, in principle, be solved by a limited number of constructive steps. A general understanding of the nature of these constructions allowed any practitioner to quickly and efficiently reach his goals, in a way that guaranteed the correctness of his results – turning the practice of measurement into a true art. But, if one wanted to move beyond the domain of spatial measurement, one needed further guidance to effect the appropriate constructions. In the case of astronomy, e.g., one had to assume that specific geometrical constructions could encode the properties of astronomical motion that were of interest. Here, I briefly consider two other domains in which sixteenth-century mathematicians were actively seeking ways to extend their problem-solving techniques.

First, let us consider mechanics, the field of practical operations involving the weighing and moving of heavy bodies. The publication of the Latin translation of Archimedes' treatise on the *Equilibrium of Plane Figures* in 1544 was an important event, but it is important to stress that this text remained completely silent on its relevance to any practical challenges. This relation was only elaborated in Guidobaldo del Monte's *Mechanicorum liber* in 1577 and Simon Stevin's *Weeghconst* (The art of weighing) and *Weeghdaet* (The practice of weighing) in 1586.[15] Stevin's treatise was explicitly introduced as the first successful reduction to art of mechanics (a relation that was also signaled by the titles of its parts). Guidobaldo's was modeled on the presentation of mechanics in the eighth book of Pappus's *Mathematical Collections* (originally written in the fourth century AD, and printed in Latin translation in 1588 under Guidobaldo's supervision), which had characterized mechanics as devoted to the solution of the general problem, "to move a given weight with a given force" (*datum pondus data potentia movere*). Both Guidobaldo and Stevin showed how this problem could be constructively solved by building a Euclidean-style framework that identified as the elementary operation the suspension of any body in indifferent equilibrium, the possibility of which could be expressed by ascribing to each body a unique center of gravity (given as the first postulate of Guidobaldo's work and as the first practical operation of Stevin's *Weeghdaet* – in both cases occupying the place of the Euclidean construction of a straight line). This operation allowed the solution of the general problem, since it entailed the proportional law of the lever and, in the case of Stevin, that of the inclined plane. To show how these laws could solve

[15] See Bertoloni Meli (2006); Van Dyck (2006); Van Dyck (2021).

concretely given challenges, the treatises again offered sustained exploration of the systematic dependencies they implied, in this case between given weights, forces, and their geometrical dispositions. The elementary operation thus allowed the mathematician to construct – conceptually and materially – different kinds of systems that were guaranteed to be in equilibrium, and which could be put into motion by adding a small amount of force. (In theory, the smallest amount should suffice; in practice, friction and other impediments had to be overcome, requiring a force that varied with material circumstances.)

The second example is a field in which mathematicians had the same ambitions as in mechanics, but not the same success. The inclusion of cannons in the figures illustrating Digges's *Pantometria* (as in Figure 14.1) was not accidental, since the surveying exercises described played an important role in warfare. This naturally raised the question whether, given that the distance to a target could be geometrically determined, one could also determine how to aim the cannon to guarantee accuracy. In the second edition of the *Pantometria*, published in 1591, Thomas Digges included a short new treatise devoted to this question, following up on the 1579 *Stratioticos*, a text on military science, again partly based on his father's work, which had also included ballistics.[16] He repeatedly formulated his ambition as the reduction of the proportional relations characterizing shots to "a *Theorike* certain," but he also had to admit that he had not yet been able to "reduce that art to such perfection" as could content him.[17] The admitted imperfection of his own work did not stop Digges from criticizing his predecessors, though, central among which was the Italian mathematician Niccolò Tartaglia.

Tartaglia's *theorica* combined a "violent" straight-line motion along the direction in which the cannon is aimed; a circular middle part resulting from the modification of the straight motion by the cannonball's weight; and a "natural" motion straight downward at the end, when only the body's weight remains operative. (See Figure 14.2 for the complete diagram, which also encodes more fine-grained information concerning the distances reached.) Modern commentators often express surprise at Tartaglia's neglect of the curvature at the beginning of the motion, but Digges was happy to ignore this, as well – as a *theorica*, the geometrical construction represented empirically measurable properties of the shots (mainly range in relation to angle), it need not offer a realistic depiction of the shape (although that could

[16] See Johnston (1994, ch. 2); France (2014, ch. 2). [17] Digges and Digges (1591, 169, 191).

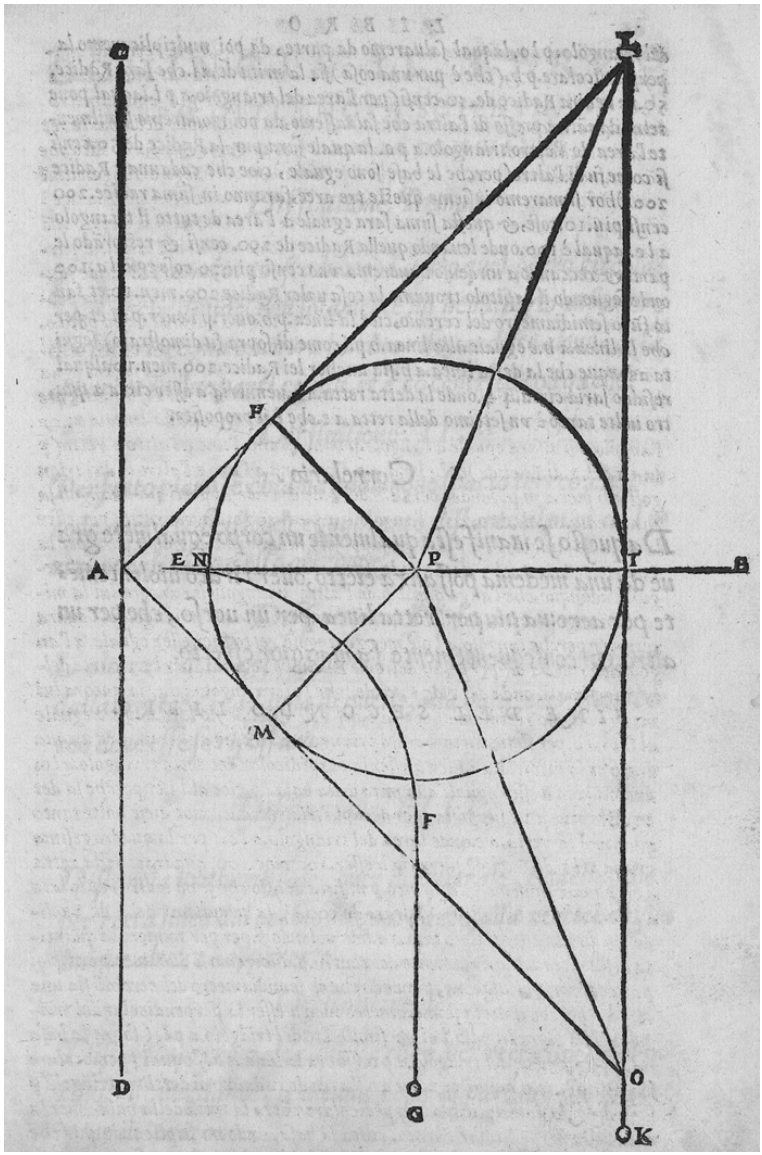

Figure 14.2. Tartaglia's *theorica*
Source: Tartaglia (1558, f. 51). From ETH-Bibliothek Zürich

be one way to guarantee correct results).[18] Digges's main criticism had to do with the circular part. The *Stratioticos* suggested that this could be rather a conic section, which would change from elliptical to parabolical to hyperbolical, depending on the angle of projection. In the second edition of the *Pantometria*, he assumed it is a "helical" line, constructed in a way similar to Archimedes' spiral. (We can understand this along the following lines, although Digges is not clear: the "natural" motion downward continually changes the direction of the "violent" motion, resulting in a continuous rotation of its line of motion, yet without changing the violent speed that needs to be superposed on the rotation.)[19] The complexity of this construction he likened to that of astronomical epicycles, eccentric deferents, and equant points. Digges clearly thought that the complexity of the problem required him to use tools beyond the strictly Euclidean toolbox taken from other works of antique geometry, such as Apollonius's *Conics* or Archimedes' *On Spirals*.

Digges was convinced that Tartaglia's *theorica* had to be misguided since it wrongly implied that the angle of maximum range should be 45 degrees, which was "an Error knowne even to the first Practitioners."[20] With hindsight it is of course tempting to see this as a mistake, but Digges was actually right in claiming that in practice shots reached their maximum range at an angle somewhere between 40 and 45 degrees – and he saw the task of the mathematician as finding a geometrical construction that could fit every bit of relevant practical knowledge, which also led him to call for further practical experiments.[21] While both authors appealed to the terminology of "natural" and "violent" motion, philosophical ideas played no direct role in Digges's criticism of Tartaglia. The distinction between the kinds of motion could be grounded in experience, and it was intuitively attractive to conceptualize the trajectory as "somehow" composed out of their combination. But the operationalization of this idea was almost completely guided by the combination of the practical knowledge at hand and the geometrical tools available, rather than by explicitly philosophical ideas – a pattern that would be often repeated in the century to follow.

[18] See Büttner, et al. (2003).
[19] This possible reconstruction of Digges's intention is missed in Johnston (1994); France (2014).
[20] Digges and Digges (1590, 358).
[21] See Büttner (2017) for an important analysis of the complicated relation to practical knowledge of early modern ballistics.

Mathematizing Physics in the First Half of the Seventeenth Century

Mathematizing Physics/Physicalizing Mathematics

The preceding has suggested geometry was not necessarily understood as a descriptive theory depicting an "abstract" reality, and that many mathematicians would more naturally see it as an art that allowed one to solve diverse problems. Petrus Ramus captured this spirit when he defined geometry as "the art of measuring well."[22] The introduction of quantitative measurement in other empirical domains could be achieved by similarly reducing them to art. The lavish, elaborate instruments (often richly and symbolically decorated) that came out of the mathematicians' workshops and were eagerly collected by the powerful stand as testimony to the fascination that could be triggered by this practice.[23] Its presence and cultural status would have an important impact on ideals about how to do natural philosophy, but this impact could take many forms. Let us start with the most familiar case: astronomy.

It is no accident Stevin and Digges were among the first committed Copernicans.[24] The main advantages of the Copernican system could be considered "instrumental" in the sense discussed above. Even if it was predictively equivalent to the family of Ptolemaic models (given the observational possibilities of the time), it encoded more systematic dependencies between the planetary motions in one geometrical model of the world-system, and this allowed the determination of relative planetary distances that had been unmeasurable.[25] The crucial step that these mathematicians took was to treat this superior instrumentality as a criterion of truth. Both Digges and Stevin were themselves too much and too proudly mathematicians to care much about the implications this had for natural philosophy, both with respect to determining legitimate criteria of truth, and with respect to the consequences a moving earth would have on ideas about motion and causality.[26] But both kinds of question would be taken up by others, and not only with respect to astronomy.

[22] Ramus (1569, 1).
[23] See, e.g., Korey (2007) for a description and analysis of one such collection.
[24] See Johnston (1994) for Digges and Vermij (2002, ch. 4) for Stevin.
[25] See Evans (1998, 410–413) for a clear and concise presentation. See also the chapter by Omodeo and Regier in this volume (ch. 13).
[26] As with the other authors treated below, the diverse factors that could explain their different stances will not be discussed here; but it is striking to what extent Digges and Stevin for the most part confidently ignored philosophy as a meaningful practice. It is

To start with the second implication, the Copernican case can be seen as an occasion for what has been usefully called the "physicalization of mathematics."[27] All accepted mathematical theories (including Euclidean geometry as a theory of spatial measurement) depended on some explicit or implicit assumptions about empirical properties of bodies (e.g., the circular motion of planets), which could be taken up in relation to what were traditionally considered to be natural philosophical questions – but which now had to be investigated with the constraint imposed by the independently assumed truth of the mathematical theories. This implied a reversal with respect to what was traditionally understood as subalternation, where mathematics was only used to derive further consequences that followed if one could assume on prior natural philosophical grounds that some empirical phenomenon could be mathematically characterized.[28]

A telling example of the opposite move of "physicalization" is René Descartes's youthful interpretation of Stevin's hydrostatic paradox.[29] Stevin had offered a mathematical treatment of the pressures exerted on the base of a vessel, in which the only properties that mattered were the weight of the fluid and the geometrical properties of the container. Descartes literally tried to fill out Stevin's geometrical diagrams by offering a corpuscular theory of matter in which pressure was interpreted as arising out of the particles' "tendency to motion" propagated through the medium. Most importantly, the laws guiding this propagation were conceived such that the mathematically established result could be recovered. To put it differently: natural philosophical notions were now *guided* by mathematically established truth.

This brings us to the first implication. My analysis has suggested that the establishment of mathematical truth depended on criteria that were directly related to superior problem-solving ability. The introduction of this kind of

also interesting to note that both mathematicians strongly believed that Copernicus could be improved upon by gathering further observations.

[27] See Schuster (2012); Schuster (2013).

[28] See Laird (1987); Distelzweig (2013). Laird (1997) already noted that the debates on subalternation could not have been of much interest to someone like Galileo, since they simply assumed what would have been the most pressing issue for him – how to characterize empirical phenomena in mathematical terms in the first place. See also Biener (2004); Van Dyck (2013).

[29] See Schuster (2012). Other examples could easily be added. To name just some of the most influential ones: Kepler's work on optics (Dupré 2012) and Gilbert's on magnetism (Bennett 2003; Johnston 2004). The philosophical elaboration of the mathematical theories' presuppositions did not necessarily have revolutionary ambitions. Guidobaldo del Monte, e.g., spent some time on showing how the science of mechanics (and more specifically the central role of bodies' centers of gravity) could be given a place within a broadly Aristotelian conception of the world (Van Dyck 2006; Van Dyck 2013).

norm into natural philosophy should perhaps be considered the primary meaning of what we can call the "mathematization of physics." Understood this way, mathematization was a process in which (practical) mathematical construction took over (at least part of) the role of the Aristotelian scheme for causal explanation. This process need not have started from prior metaphysical beliefs about the ultimate nature of reality, as Koyré famously contended, but it was rather naturally suggested by the successes and broader appeal of mathematical practice, which allowed it to serve as a new ideal for knowledge more broadly.[30] This move had two immediate consequences: it invited the reinterpretation of questions traditionally belonging to the domain of natural philosophy as new problems to be constructively solved, greatly expanding the domain of mathematical treatments (often necessitating the development of new mathematical tools); and it occasioned the development of philosophical discourses aimed at legitimating this reinterpretation (which could, but need not, appeal to explicitly metaphysical arguments). This latter step was often closely related to the physicalization of mathematics, as the discourses could offer conceptual resources for interpreting the assumptions underlying the constructions as natural philosophical principles.

These characterizations are of course very schematic, and need to be fleshed out by showing how they allow us to make sense of the work of different authors – not only what they have in common, but also bringing to light the different options made possible by the shared ideal of articulating a new physics based on the mathematical arts. The next sections will very briefly discuss three historically influential examples, without claiming that this would exhaust the field of options.

Galileo: Mathematizing the Phenomena

Soon after his appointment as professor of mathematics at the University of Pisa in 1589, the young Galileo composed a treatise on natural philosophy (commonly called *De motu antiquiora*), which he never published but which survives in a few manuscript drafts.[31] The text stridently opposed the method that Galileo claimed to have learned from "his mathematicians" to that used

[30] This was of course only "rather naturally suggested" for authors and readers with a specific background, while many other thinkers could only consider it an obvious metaphysical blunder. The important point remains that metaphysical pictures can be challenged from perspectives other than those provided by articulated alternatives. See Hatfield (1990).

[31] See Fredette (2001).

by the traditional philosophers.[32] The result can somewhat provocatively be characterized as an attempted reduction to art of the philosophical science of local motion. One of the manuscript notes consists of a list of some of the most important questions treated in the debate on motion among Pisan philosophers,[33] and we can read the treatise as trying to show that these could all be constructively solved using Archimedean mathematical tools.[34] Hydrostatics offered Galileo a scheme of systematic dependencies between weights and volumes of bodies and of the medium in which they move that allowed him to determine under which circumstances bodies move up or down. In a crucial extrapolation, he suggested that these dependencies could also solve a further problem (the formulation of which brings to mind Pappus's central problem of mechanics): "to give the speed of motion given the weights of a body and the medium" (*data gravitate mobilis et medii, datur velocitas motus*).[35] His suggestion was to set the velocity equal to the hydrostatic force, which could already be determined. We can see Galileo tentatively attempting a construction based on the models already at his disposal, again not dissimilar from the way that Tartaglia and Digges were exploring possible *theoricae* for projectile motion.

The preceding description could misleadingly suggest that Galileo's text was presented as a treatise in mathematics, but the mathematical constructions actually formed the backbone for an exercise in "physicalization." Galileo used the constructions to rethink the crucial natural philosophical concepts implicated in the formulation of the original questions, which also guaranteed that the constructive solutions could be presented as "answers" to these philosophical questions. The opening of the treatise accordingly tied the meaning of "heaviness" and "lightness" to conditions of measurement – in a way that allowed the problems formulated in terms of heaviness to be in principle solvable. This, in turn, suggested some options to reconceptualize the notions of "natural" and "violent" motion, options that were progressively worked out in the different drafts, and which also led to the suggestion to categorize horizontal motion as neither natural nor violent but "neutral."[36]

[32] Galilei (1890–1909, 1:285). [33] Galilei (1890–1909, 1:418).

[34] See Camerota and Helbing (2000) for the Pisan context. It is very suggestive to compare Galileo's list with the comparable list of more practically oriented questions concerning ballistics that Thomas Digges had included in his *Stratioticos* as a preliminary step to the reduction to art.

[35] Galilei (1890–1909, 1:418). [36] Galilei (1890–1909, 1:300).

In the decade following this first attempt, Galileo would reshape both his mathematical constructions and the accompanying philosophical interpretations, based on new empirical findings triggered by some of the problems treated in his early treatise. In a first experiment, Galileo found that the trajectory of a projectile is approximately parabolical.[37] In a second experiment, he found that a simple pendulum is approximately isochronous. The relevance of this latter phenomenon was probably brought to his attention by his analysis of motion on inclined planes, already included in a chapter of *De motu antiquiora*, and it further invited the search for a mathematical construction of this empirical finding based on an approximation of circular motion by motion over a sequence of inclined planes. The latter search could be further guided by the results of a third experiment, in which Galileo famously established the times-squared law of free fall by rolling balls down inclined planes.[38]

This last finding had at least two important consequences. First, it set a new fundamental problem to be solved: how to geometrically model accelerated motion? This would bring Galileo into uncharted mathematical terrain, since the toolbox of antique mathematics could not provide him with the instruments he needed to represent the systematic relations between distances, speeds, and times he was in the process of uncovering.[39] Second, it invited a further philosophical reconceptualization of motion. Galileo now had to find a way to present accelerated motion as natural, and this gave rise to an even more far-reaching consequence once related to the first experimental finding. If distances were as the squares of times in free fall, then the parabolic shape of the trajectory of a projectile could be directly constructed from the composition of free fall with a horizontal component that was uniform. Assuming the latter allowed Galileo to answer a question that had already arisen in *De motu antiquiora*, but which had remained unanswered:

[37] See Renn, et al. (2000) for the experiment and its relation to the theory of *De motu antiquiora*. It is important to notice that Galileo could find this out by taking the question out of the realm of ballistics proper: rather than experimenting with actual artillery (as Digges claimed to have done), Galileo and his collaborator Guidobaldo del Monte simply threw an inked ball along a plane that was tilted with respect to the horizon and inspected the trajectory drawn by the ball itself. Arguably, it is only the natural philosophical context (in which bodies in violent motion were typically assumed to be thrown) that could have suggested the relevance of this kind of situation for the more general question. See Büttner (2017) for the very limited relevance of Galileo's finding for the practice of artillery.

[38] See Wisan (1974); Büttner (2019).

[39] See Damerow, et al. (2004) for an analysis of Galileo's attempts to come to terms with this problem.

whether a "neutral" motion once started would persist or end.[40] The positive answer to this question would form the kernel of a new approach to the analysis of motion, as it can be seen to play a role related to what would become the inertial principle. (It also allowed Galileo to partly meet the challenge of physicalizing the Copernican hypothesis.)[41] We again find the same move: a natural philosophical concept was given empirical content by tying its meaning to the requirements of mathematical construction. But Galileo was well aware that his conceptual apparatus was still incomplete, since he was not able to fully identify the concepts that could play the role that the center of gravity had played for mechanics, expressing the relevant empirical property that could satisfactorily ground the mathematical superstructure.[42]

When Galileo presented the final results of his investigations in *Two New Sciences* in 1638, the constructions were explicitly presented as forming an elaborate and self-contained mathematical treatise on motion in Latin. This was still embedded in a broader philosophical discourse, in the form of an Italian dialogue, but the nature of the discourse had shifted significantly compared to *De motu antiquiora*. It was no longer intimately tied to a philosophical agenda that had been set by Aristotelian natural philosophy. Motion as the treatise's topic was simply presented as an "ancient subject" about which much had been written.[43] Some of the discussions of *De motu antiquiora* were taken up again, but they were integrated into a more exploratory discussion of many different topics that all shared a relation to mathematical problems, an approach that was explicitly justified by a reference to "the richness joined with great liberality of nature"[44] that allowed ever new discoveries to be made – a process for which mathematics with its constructive capacity for infinite invention based on the study of systematic dependencies was eminently suited.[45]

The discovery of mathematical regularities characterizing acceleration and pendular motion had brought Galileo to a position that saw mathematizability of phenomena as the main criterion for inclusion in natural philosophy. Philosophical enquiry became an open-ended search into the richness of nature, with no *a priori* guarantee that everything could be treated

[40] See Van Dyck (2018) for further analysis.
[41] See Roux (2006) for a careful treatment of the pre-history of the inertial principle.
[42] See Wisan (1974); Damerow, et al. (2004); Büttner (2019) for the unfinished nature of Galileo's mathematical framework for dealing with the phenomena of motion.
[43] Galilei (1890–1909, 8:190, 266–267). [44] Galilei (1890–1909, 8:140).
[45] Galilei (1890–1909, 8:267).

mathematically – but with the norms of success straightforwardly defined by mathematical practice, where the material instrumental infrastructure of the latter now included pendula and inclined planes besides quadrants and balances.

Descartes: Mathematizing the World

While Galileo's endeavor started from an approach to mathematics informed by the eighth book of Pappus's *Mathematical Collections* (as further developed by Guidobaldo del Monte), Descartes's wider program can be seen to start from Pappus's fourth and seventh books, with their discussions of the appropriate means of geometrical construction and the analytic method in mathematics. As has been well documented, the young Descartes was fascinated by the prospects of an absolutely general problem-solving scheme involving a central place for mathematical instruments.[46] Significantly, he expected this scheme to be directly relevant to natural philosophical problems, which should thus be constructively solved.

In the decade after the first letters and notes (from 1619), Descartes kept returning to mathematical and natural philosophical problems. His work on optics was initially in line with other approaches to applied mathematics at the time. By analyzing the systematic dependencies encoded in a geometrical model that had been constructed taking into account empirical observations, Descartes was able to establish the law of refraction.[47] At the same time, he was keen on reinterpreting these constructions in natural philosophical terms as "following" from hypotheses concerning the nature of light (whereas, in a familiar move, these hypotheses could only be operationalized by first assuming the geometrical constructions). This physicalizing step was further foregrounded and given an explicitly metaphysical foundation by the end of the 1620s, when Descartes started developing metaphysical ideas implying a limitation on possible modes of constructing explanations of all natural phenomena.[48] Interestingly, this was paralleled by a similar development in his work on pure mathematics, where he also came to a position implying a principled distinction between legitimate and illegitimate modes of constructing curves for solving geometrical problems that went beyond the strictly Euclidean canon.[49]

[46] See Bos (2001); Sasaki (2003) for a general introduction. See also the chapter by Guicciardini in this volume (ch. 15).
[47] See Schuster (2012); Heeffer (2017) for possible reconstructions.
[48] See Garber (1992) for a classic study of Descartes's "metaphysical physics."
[49] See Bos (2001) and Domski (2009), which focuses on the parallel role of motion in both natural philosophy and pure mathematics.

Descartes's focus on absolute universality set his work clearly apart from that of Galileo. At the latest by *1633*, when he finished work on *Le monde*, Descartes believed he had been able to metaphysically determine a fixed basis of operations that would suffice for the constructive explanation of everything that happened in nature, and this allowed him to resolutely set himself apart from more piecemeal empirical and mathematical exploration as the guide to the selection of both the mathematical basis and the phenomena to be constructed. This especially comes to light in Descartes's critical reaction to Galileo's law of free fall.[50] Descartes was convinced that this mathematical regularity could only be considered an approximate characterization, valid under limited and contingent circumstances and with no wider relevance, since it could not be squared with a construction of acceleration as due to consecutive pushes by rotating subtle matter that should explain gravity (an empirical property of bodies simply assumed by Galileo). Prioritizing a metaphysical norm that had initially been suggested by mathematical practice, Descartes was now overruling a mathematical characterization of phenomena as not part of proper physics. The metaphysical nature of matter as pure extension and of change as local motion, with collision the only form of interaction, constrained all possible models, rather than independently established mathematical models constraining philosophical pictures of nature. The empirical world had been made co-extensive with the subject-matter of mathematics, but in the same move the practice of physics had become metaphysical.

Mersenne: Mathematizing the Scientific Community

It is no coincidence that Descartes's comments on Galileo's work had been solicited by Marin Mersenne. The French polymath's personal and correspondence network played a crucial role in constituting the beginning of an institutional space for the new approaches in mathematics and philosophy mainly being developed outside the universities.[51] Beside Descartes, it included mathematicians and philosophers like Isaac Beeckman, Pierre Gassendi, Gilles Person de Roberval, Étienne and Blaise Pascal, Pierre de Fermat, Claude Mydorge, Girard Desargues, Jean de Beaugrand, Jean-François Niceron, Honoré Fabri, Thomas Hobbes, Giovanni Battista Baliani, Bonaventura Cavalieri, Evangelista Toricelli, and Christiaan Huygens, offering an amazing cross-section of the mid-century European

[50] See Palmerino (1999, 282–295) for discussion.
[51] For the characterization of the network as an institution, see Goldstein (2013). See also Grosslight (2013).

mathematical landscape. Through this network, application of mathematics started to become a more communal process. Mersenne had seen that constructing a community could be integral to the construction of solutions to particular and general problems.

Mersenne's letters and publications (among which were translations and summaries of Galileo) were important in reporting ideas, techniques, results, and empirical data. They allowed the construction of a compendium of physico-mathematical problems (to use a term favored by Mersenne) deemed important in the period: free fall and motion on inclined planes, pendular motion (including that of rigid bodies), ballistics, simple machines, music, optics and catoptrics, hydrostatics and pneumatics (with an important place for the alleged existence of a void), hydrodynamics, the force of percussion, the strength of materials, etc.[52] Mersenne's role was much more active than that of a mere informational nexus, though. He continually challenged his correspondents with new problems, and as the exchange with Descartes on Galileo's work illustrates, these challenges typically also included requests to critically assess the work of other mathematicians. Mersenne's own relatively uncommitted stance on many matters made him ideally suited to play the role of mediator (and vice versa – the way he saw his role probably drove his stance).[53]

The community constructed by Mersenne was not held together by common ideas about the mathematization of physics or by widely accepted results. The application of mathematics to new problems forced everyone involved to rethink the proper place of experience and philosophical interpretation or supplementation, and to try out new mathematical techniques that could in turn be contested. Almost nobody agreed on the answers, but everyone recognized what was at stake – and it is primarily around this recognition that the fledgling disciplinary community was being built.

Conclusion: Physics as Problem-Solving

When Isaac Newton published his *Mathematical Principles of Natural Philosophy* in 1687, he could straightforwardly claim in his Preface that "the whole difficulty of philosophy seems to be to discover the forces of nature

[52] For a discussion of most of these problems, see Bertoloni Meli (2006), a work on the history of seventeenth-century mechanics that seems to have been written very much in Mersenne's spirit.

[53] On Mersenne's stance, see Dear (1988); Garber (2004). This did not mean that Mersenne did not take a position on important issues. See Palmerino (2010).

from the phenomena of motions and then to demonstrate the other phenomena from these forces."[54] The fundamental natural philosophical challenge had become a problem (the formulation of which again distantly recalls Pappus) that was to be mathematically solved. Galileo's metaphor had depicted the universe not only as a book but also as a labyrinth; Newton proposed his new concept of force as Ariadne's thread with which to construct clear paths that made nature legible. A new ideal of knowledge was taking hold according to which knowing nature implied being in a position to gradually find out more by constructively exploiting the information at one's disposal and making it fruitful for further exploration.

The practice of "mathematical physics" (as it would become known) also triggered important developments in pure mathematics that extended and transformed Descartes's analysis of the conditions under which geometrical problems could be solved. By the end of the century, the calculus had established itself as a general analytic scheme that could, in turn, be justified by its use in solving important physical problems.[55] Solving differential equations would become the core of a whole new approach to the study of systematic dependencies that far outstripped what could be expressed by geometrical proportions. This created a mathematical setting where many of the problems already raised by Mersenne, such as the rotation of rigid bodies, the vibrations of a taut string, or the behavior of jets of waters, found novel solutions that moved physics decidedly away from Newtonian methods and concepts.[56] Yet how to philosophically interpret these later successes remained an open problem.[57]

[54] Newton (1999, 382).

[55] See Mahoney (1990a) for an analysis of the "bootstrapping" process by which physical and mathematical problem-solving mutually shaped and supported each other.

[56] See the chapters by Stan (ch. 21) and Hepburn and Biener (ch. 16) in this volume.

[57] I would like to thank the editors for their feedback, which helped clarify my presentation. Thanks to Joel A. Klein for help in obtaining images.

15

Mathematical Innovation and Tradition: The Cartesian Common and the Leibnizian New Analyses

NICCOLÒ GUICCIARDINI

During the seventeenth century, the advent of what were known as the "common" and "new" analyses fundamentally changed the mathematical landscape of European mathematics. The widely accepted narrative is that these seventeenth-century analyses, algebra and calculus, mostly due respectively to René Descartes and Gottfried Wilhelm Leibniz, occasioned a transition from geometrical to symbolic methods; i.e., from Euclidean proportion theory to equations.[1] In dealing with the science of motion, statics, dynamics, optics, and other mixed mathematical sciences, mathematicians abandoned the language of proportion theory, as found in Galileo Galilei's *Discorsi* (1638), Christiaan Huygens's *Horologium oscillatorium* (1673), and Isaac Newton's *Principia* (1687), and began employing algebraic equations and, later in the 1690s, Leibnizian differential and Newtonian fluxional equations. This was the advent of a more abstract way of practicing mathematics, which culminated with the algebraic approach to calculus and mechanics promoted by Leonhard Euler and Joseph-Louis Lagrange in the eighteenth century.[2] In this chapter, I will show that geometrical interpretations and mechanical constructions still played a crucial role in the mathematical methods of Descartes, Leibniz, and their immediate followers. This is revealed by the manner in which they handled equations and how they sought their solutions. Moreover, the passage from proportions to

[1] One can read in Morris Kline's influential and still fundamental textbook: "Whereas from Greek times until about 1600 geometry dominated mathematics and algebra was subordinate, after 1600 algebra became the basic mathematical subject; in this transposition of roles the calculus was to be the decisive factor" (Kline 1971, 323).

[2] See the chapters by Stan (ch. 21) and Hepburn and Biener (ch. 16) in this volume.

differential equations did not occur in a single step; it was a gradual process that took about a century to reach completion.

Equations and Solutions in the Cartesian Common Analysis

Descartes's *Géométrie* (1637) brought about a new way to conceive relationships between algebra and geometry. The innovation concerned the geometrical interpretation of algebraic operations and the algebraic representation of plane curves. Before Descartes's innovation, if a and b are two segments, ab was interpreted as the construction of a rectangle, a "plane," having a and b as sides, while abc was understood as a "solid." Moreover, as François Viète argued in his *In artem analyticem isagoge* (1591), in writing an equation, only terms of the same dimension could be summed, so that it was permissible only to sum a segment and a segment, a "plane" and a "plane," etc. Descartes, however, explained how operations on segments could be defined as closed operations, so that not only $a + b$ and $a - b$ (when $a > b$), but also ab and a/b are segments. Specifically, Descartes introduced a unit segment e, so that $e{:}a = b{:}ab$.

> For example, let *AB* [in Figure 15.1] be taken as unity, and let it be required to multiply *BD* by *BC*. I have only to join the points *A* and *C*, and draw *DE* parallel to *CA*; and then *BE* is the product of *BD* and *BC*.[3]

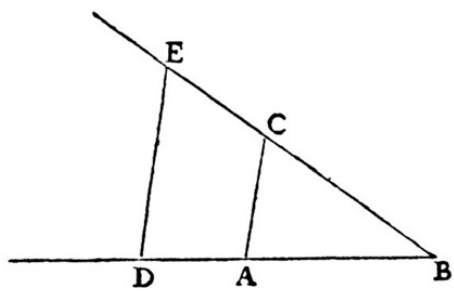

Figure 15.1. Descartes's geometrical interpretation of algebraic operations
Source: Descartes (1637b, 298)

[3] Descartes (1637b, 298).

In other words, once a unit segment (in this case, *AB*) is chosen, the product of two segments is represented by another segment (*BE*), not by a plane surface. Similarly, Descartes showed how one could interpret a/b and the square root of a as segments.[4]

Furthermore, Descartes studied plane curves via equations representing the relationship between their *ordinatae* and *abscissae*. Descartes did not use "Cartesian" axes, but defined the coordinates intrinsically – i.e., by referring them to the configuration of a geometrical figure. Thus, the generic point *C* belonging to a plane curve, a locus of points satisfying some geometrical condition, was identified with the algebraic relation between two, generally non-orthogonal segments embedded in the figure at hand, which Descartes called the *indeterminées*. The study of curves and their properties could thus be translated into a study of their defining algebraic equations. For example, in the case of the ellipse (see Figure 15.2), Descartes considered an arbitrary point *C* on the ellipse, and projected it as point *M* on the abscissa *AG*. (Note that the ordinate *MC* is erected obliquely.) The *indeterminées* are the length of the segment $AM = y$ and the length of the segment $MC = x$. By referring to Proposition 13, Book 2, of Apollonius's *Conics*, Descartes states that the defining property of the ellipse translates into the following equation:

$$xx = ry - (r/q)yy$$

where x and y are what we would call the coordinates of point *C*, and r and q are two constants (the *latus rectum* and the *latus transversum*).[5]

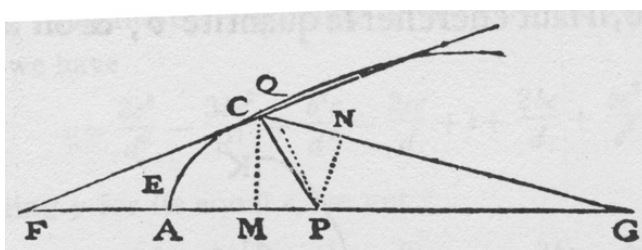

Figure 15.2. Algebraic definition of an ellipse
Source: Descartes (1637b, 342)

[4] Bos (2001). [5] Descartes (1637b, 342).

Representing curves this way brought the generality of algebra into the specificity of geometry. While the Greeks had studied individual "named" curves (the circle, the parabola, the cissoid, etc.), the new Cartesian algebraists studied *classes* of curves. In Descartes's *Géométrie*, all the conics (circle, parabola, hyperbola, and ellipse) can be dealt with in terms of second-degree polynomials in two *indeterminées* (as with the ellipse). The same holds true for cubics, quartic curves, and so forth.

The *Géométrie* is thus often seen as marking the birth of analytic geometry. However, the algebraic methods developed by Descartes have their own peculiarities and their own limitations that distinguish them from analytic geometry as we would understand it nowadays.[6] Indeed, Descartes devoted many pages of the *Géométrie* to describing not so much the *equations* that define curves, but rather several *instruments*, consisting of linked sliding rulers and strings, tracing curves by continuous motion. These include the *mesolabum* (see Figure 15.3), wherein the arrangement of sliding rulers is such that the points *D*, *F*, and *H* (when the angle *AYB* increases from zero) describe the

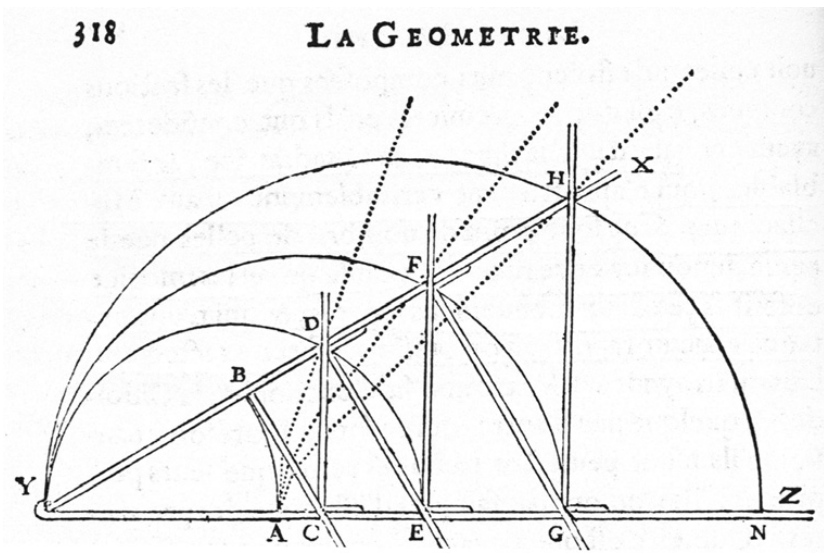

Figure 15.3. Descartes's mesolabum
Source: Descartes (1637b, 318)

[6] Bos (1998).

dotted curves.[7] What would be the role of these tracing mechanisms in a work on analytic geometry?

As Bos shows, the answer is that the study of plane curves is not the only issue for Descartes. Or better, curves are not only the *objects* studied in the *Géométrie*, but also the *tools* employed for geometrical constructions. Descartes's method was an answer to issues stemming from a general survey of ancient Greek mathematics composed by Pappus in the fourth century, which appeared in Latin translation as *Collectiones mathematicae* in 1588. In the seventh book, Pappus presented the double method of analysis and synthesis. Analysis, he wrote, was conceived of as a method of discovery, or a method of problem solving, which, by working step-by-step backward from what is sought as if it had already been achieved, eventually arrives at what is known. Synthesis goes the other way around, starting from what is known and, working through the consequences, eventually arriving at what is sought. Analysis (in Latin, *resolutio*) was often thought of as a method of discovery preliminary to the synthesis (*compositio*), which, reversing the steps of the analysis, achieves the true demonstration.

In the decades following the Latin publication of the *Collectiones*, belief in the existence of a lost, or hidden, method of discovery – a "Treasure of Analysis" that allowed the ancients to achieve extraordinary results – promoted many efforts aimed at "restoring" the ancients' method of discovery, the *analysis Veterum*.[8] Symbolic algebra could be conceived of as "analysis" by many of its early promoters because, as Descartes claimed in the *incipit* of the *Géométrie*, it "resolves" problems by means of equations in which names are given to all

[7] Descartes (1637b, 318).

[8] The project of restoring the lost books by Euclid and Apollonius, according to Pappus's description of the "method of analysis," occupied mathematicians for many generations, from Federico Commandino's school in Urbino in the mid-sixteenth century; to Marino Ghetaldi, Frans van Schooten, and Newton in the seventeenth century; to the eighteenth-century efforts of Robert Simson and Matthew Stewart. Indeed, even in the nineteenth century, Michel Chasles considered this project interesting, and the great editions of the likes of Johan Ludvig Heiberg can be viewed as a continuation of this project. This is a field in which philology and mathematics interacted in a complex way and in which the belief in a method of mathematical discovery of the ancient Greeks, to be recovered from extant texts, was often accompanied by the appreciation of the novelty of algebra and calculus, the analyses of the moderns. One might mention that the three great protagonists in the application of algebra to geometry, Viète, Descartes, and Pierre de Fermat, took the confrontation of modern algebra with the lost "analysis of the ancients" quite seriously. Viète depicted his *In Artem analyticem isagoge* as a work in which the analysis of the ancients was both restored and extended (Viète 1591, 1[r]). Similar concepts were expressed by Pierre de Fermat in *Ad locos planos et solidos isagoge* (Fermat 1679, 1) and by Descartes in *Meditationes de prima philosophia* (1641) (Descartes 1964–1974, 7:155–156).

quantities, known and unknown.[9] When we translate a geometric problem in terms of an equation, Descartes observed, we suppose the solution already achieved, as in the ancient method of analysis described by Pappus.

However, the important contribution of the *Géométrie*, namely the translation of geometric problems into algebraic equations, is not the end of Descartes's method for problem solving. Such a translation constitutes just the "resolution" of a problem, not its "solution"; the "analysis," not the "synthesis." The algebraic calculation of the roots of an equation does not yield, in Descartes's mind, a solution to a geometric problem, as it might happen nowadays. According to Descartes, the solution of a geometric problem must end with a geometric *construction*. And in order to thus "construct an equation," points must be determined via intersection of curves traced by continuous motion, yielding line segments with lengths equal to the roots of the equation (given that a unit segment has been previously defined). As in Euclid's *Elements*, where problems are constructed by intersections of circles and straight lines traced by compass and straightedge, so in the *Géométrie* problems are constructed by intersections of plane curves, such as ellipses, hyperbolas, and higher-order algebraic curves, traced by instruments of Descartes's conception.

We can see this Cartesian style of analysis and synthesis at work in a simple problem taken from the *Géométrie*: the trisection of an angle. Here, one seeks to divide a given angle *NP* into three equal parts; that is, the length of the chord *NQ* (in Figure 15.4) must be found given the length of the chord *NP*. To find the solution, Descartes first resolves the problem into a third-degree algebraic equation in one unknown. He names $NO = 1$ (that is, he chooses as unit segment the radius of the circle), $NP = q$ (the chord of the given angle), and $NQ = z$ (the chord to be found). From the similarity of triangles *NQO*, *QRN*, and *RSQ*, he obtains that *NO* is to *NQ* as *NQ* is to *QR* as *QR* is to *RS* (*NO*:*NQ*::*NQ*:*QR*::*QR*:*RS*). From this proportionality, an equation follows:

$$z^3 = 3z - q \tag{1}$$

Once he has found equation (1), Descartes does not seek to calculate its roots. Rather, he provides a geometric construction as a solution of the trisection problem; a construction which resembles similar techniques developed long before in Islamic mathematics.[10] Descartes "constructs the equation" by an

[9] Descartes (1637b, 300).

[10] Knorr (1989) contains most of the Arabic texts on the trisection of the angle. Some of the Arabic techniques bear a resemblance to Descartes's. Most notably, Omar al-Khayyam (1048–1131) developed techniques of construction for third-degree equations via

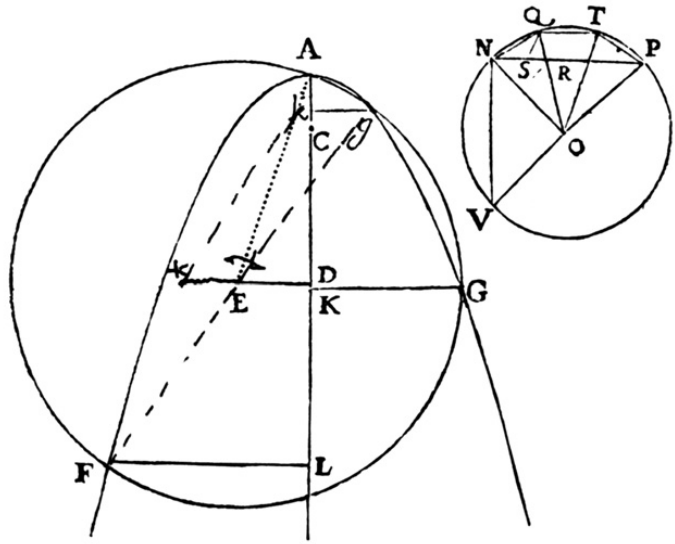

Figure 15.4. Diagram for the problem of angle-trisection in Descartes's *Géométrie*
Source: Descartes (1637b, 298)

intersection of circle and parabola. Given a fixed parabola of *latus rectum* equal to 1, Descartes determines (in a function of the coefficients of the equation) the radius and the position of the center *E* (in Figure 15.4) of a circle that intersects the parabola in four points: *A* (the vertex), *g*, *G*, and *F*. The ordinates *kg*, *KG*, and *FL* have lengths equal to the two positive roots and one negative root of the equation. The segment *kg* is the sought chord *NQ*, which thus is constructed mechanically by tracing the circle and the parabola.

The Cartesian technique can be contrasted with what we would do today. Nowadays, we would seek the solution of the trisection problem by calculating the roots of equation (1). If a geometric representation of the roots were required, we would seek the intersection of the graph of $y = z^3 - 3z + q$ (a cubic curve) with the z-axis (a straight line). But Descartes is not thinking about a solution in numerical, but strictly geometrical terms. Rather than deploying the intersection between a cubic and a straight line, Descartes prescribes the use of two conics (a circle and a parabola), which are simpler to trace than a cubic. Indeed, a mechanism for tracing a cubic is going to be

intersection of conics. See Rashed and Djebbar (1981). It is interesting that Descartes corresponded on mathematics with Jacob Golius, the avid manuscript collector and Semitist who held the chairs of mathematics and Arabic in Leiden.

more complex than the parabolograph and the compass deployed in Descartes's example. The point to be stressed here is that Descartes is seeking solutions to geometrical problems in terms of constructions. His method, what was then called "common analysis," prescribes how to translate geometrical problems into algebraic equations, but the solutions are not provided in algebraic language; they are geometrical constructions. As we shall see in the next section, this continued to be true for Leibnizian differential equations.

The prominence of geometry in Cartesian "common analysis" in the period of our interest is also apparent when considering how equations were written by Descartes and by other practitioners of algebra. Specifically, they were written as "homogeneous" equations (according to the early modern definition). In homogeneous equations, only magnitudes of the same dimension – i.e., homogeneous magnitudes – can be added to or subtracted from each other. To clarify this idea: $b^2 + c = 0$ is not a homogeneous equation, because on the left-hand side there is a sum of a second-degree term and a first-degree term. By contrast, $b^2 + ac = 0$ is a homogeneous equation.[11]

Consider equation (1) again. It takes its form because a unit segment $NO = 1$ has been chosen. If the choice of a unit segment had been avoided, and the radius simply named $NO = r$, we would have obtained an equation in which all terms have the same dimension (in Viète's terms, they are all "solids"):

$$z^3 = 3r^2z - r^2q \tag{2}$$

Equation (2) means that a cube with side z has the same volume as the difference between two parallelepipeds of sides $3r$, r, and z, and r, r, and q, respectively. From this example, we learn that a geometric derivation based on proportionality always leads to an equation in which all terms have the same dimension. According to early modern terminology, equation (2) is "homogeneous," while equation (1) is not.[12]

In the period considered in this chapter, the requirement of homogeneity was very important and was explicitly called into use. Polynomials preferably were written as homogeneous equations (equations in which all terms have the same dimension): thus, (2) would have been preferable over (1). This

[11] Today, since b and c are constants, b^2, c, and ac would be considered zero-degree terms.

[12] Today, a "homogeneous polynomial" is one in which all nonzero terms have the same degree, where the degree of a term is the sum of the exponents of the variables in that term. Thus, from the contemporary standpoint, z^3 is a third-degree term, $3r^2z$ is first-degree, and the degree of r^2q is zero.

practice continued well into the eighteenth century. For example, Gabriel Cramer, in his wonderful treatise on algebraic curves of 1750, still writes all his equations as homogeneous equations.[13] Indeed, very much as in Viète's work, the multiplication of symbols was defined as leading to higher-dimensional magnitudes. That is to say, if *a*, *b*, and *c* stand for line segments, then *ab* stands for a surface and *abc* for a solid, and it makes no sense to add a segment to a solid, or a surface to a solid, etc.

As we have seen in this section, in the *Géométrie* Descartes explained how operations on segments could be defined as closed operations, so that if *b* is a segment, b^2 is a segment, too. Thus, for Descartes, $b^2 + c = 0$ would be a meaningful equation – it is the sum of two segments. It should be noted, however, that even in the *Géométrie*, Descartes preferred writing equations (in his case, polynomial equations in one or two *indeterminées*) in accordance with Viète's principle of homogeneity, especially when he was dealing with applications to geometry.[14] Descartes departed from homogeneity only when he dealt with the theory of equations, most notably when he considered how to reduce them to canonical form, or when he studied the number of roots. In these more abstract parts of the *Géométrie*, in which (it seems) a numerical interpretation prevailed, one can find non-homogeneous equations.

But Leibniz castigated Descartes's even occasional departures from homogeneity in a 1698–1699 manuscript on the *mathesis universalis*:

> And even though Descartes was in the habit, with not-infrequent zeal, of violating the law of homogeneity [*lex homogeneorum*; literally, law of homogeneous magnitudes] by the introduction of a unit, I do not find a great advantage in this practice, and rather prefer, following Viète, insofar as this is possible in a convenient way, to follow the law of homogeneity, even when dealing with numbers. Indeed, the calculus ensues in a very spontaneous way, and corresponds in the greatest degree to the order of things. Mistakes are more easily avoided, because it is convenient to follow the law of homogeneity when checking the calculations.[15]

Thus, Leibniz claims, writing equations in accordance with the law of homogeneity follows from the fact that algebra expresses meaningful relationships; they "correspond to the order of things." Further, adhering to this *lex homogeneorum* allows one to spot eventual mistakes, which are due to imbalanced reckonings. Leibniz's law was relevant to a number of issues concerning the meaning of algebraic symbolism and the construction of

[13] Cramer (1750). [14] See, e.g., Descartes (1637b, 299).
[15] Leibniz (1962c, 65–66). See also Leibniz (2018, 145).

solutions. Leibniz's insistence on it also shows that, for the mathematicians of the seventeenth century and well into the middle of the eighteenth century, algebraic symbolism had to be interpreted geometrically, not numerically, and solutions were sought as constructions.

Now we are in a position to appreciate the peculiarities and limitations of Descartes's *Géométrie*. Rather than representing a step toward the liberation of geometrical method in favor of algebra, Descartes's masterpiece delivers an algebraic method of discovery that must be followed by a geometric construction, hence the importance of tracing devices capable of mechanically generating algebraic curves. The limitation here is that Descartes confined his method to polynomial equations, such as equation (1), which implies that he could not deal with transcendental curves. One of the reasons why the calculus was necessary to the mathematicians of the Scientific Revolution is that the problems they had to deal with, from Pedro Nunes's loxodrome to Blaise Pascal's cycloid, from Leibniz's catenary to Johann Bernoulli's brachistochrone, involved the use of transcendental curves. The infinite and infinitesimal, the innermost chambers in Leibniz's labyrinth of the continuum, thus entered the scene of mathematics, at least in part in order to tackle problems that originated in the project of reading the book of nature in mathematical terms.

Solutions as Constructions in the Leibnizian New Analysis

If one were to indicate another "inflection point" in the development of early modern mathematics, one would inevitably refer to the publication of Leibniz's papers on the differential and integral calculus in the 1680s. Leibniz's analysis, which often was contrasted with the Cartesian "common analysis," was "new" because it was not confined to finite polynomials; it dealt with infinite series and handled infinitesimal magnitudes. Very soon after its introduction, a network of highly skilled mathematicians, mostly active in Paris and Basle, began further developing the Leibnizian "new analysis." Among the first practitioners of the Leibnizian calculus were Jacob and Johann Bernoulli and Jacob Hermann in Basle, and Pierre Varignon and Guillaume de L'Hospital in Paris.

The differential and integral calculi have been depicted, and not without reason, as the realization of a passage from geometry to symbolism, from mechanical construction to algorithm.[16] Leibniz, the great

[16] This thesis is authoritatively defended in Breger (1986); Breger (2008).

mathematician-cum-philosopher who contributed most to their promotion in the republic of letters, indeed saw the calculus as a first implementation of a *characteristica universalis*, an algebraic language, that would be able to reduce all reasoning to *cogitatio caeca*, "blind" computation.[17] But, as in the case of Descartes, we are deceived by the apparent abstraction of Leibniz's analysis. We risk projecting back onto Leibniz and his followers a modernity of outlook that was extraneous to their mathematical culture. Geometry and mechanical constructions continued to play a prominent role in their mathematical practice, very much as in Descartes's *Géométrie*.

To convince ourselves about the importance that geometrical and mechanical interpretations still had in the early stages of the development of the calculus, it is useful to focus our attention on the field of investigation most important for the Leibnizian network active in the decades between 1690 and 1720: ordinary differential equations. Research on the calculus by Leibniz, Varignon, Hermann, and the Bernoulli brothers found its maximal realization in the application of ordinary differential equations to problems of mechanics, optics, and geometry. In the final decade of the seventeenth century and the first two decades of the eighteenth, the treatment of problems – such as the brachistochrone, the *elastica*, the catenary, and orthogonal trajectories – led to the formulation and solution of differential equations using methods that were to enjoy enormous success. Indeed, these methods still are offered in the first pages of integral calculus textbooks.

But what were differential equations for the early practitioners of the Leibnizian calculus? How did they view them? Today, an ordinary differential equation is defined as an equation in which an unknown real-valued function of a real variable and its derivatives occur. To solve, or to "integrate," an ordinary differential equation means finding an algebraic expression of a family of curves, each a particular solution of the equation for specified initial conditions. That is to say, the solution of a differential equation consists of obtaining a family of functions $f(x, y, a, b, \ldots) = 0$, each of which solves the equation, and wherein the parameters a, b, etc. are expressed as a function of the initial conditions. For Leibniz and his students, however, this is not how things were understood.

Dominique Tournès dwells on the importance of mechanical constructions for the integration of differential equations in the Leibnizian school.[18] He shows how solutions, especially when provided by transcendental functions, were sought in terms of geometric constructions by Leibniz and the

[17] Mugnai (2015). [18] Tournès (2009).

Leibnizian mathematicians. Indeed, it is only in the middle of the eighteenth century that mathematicians learned to express trigonometric functions algebraically.[19] This is interesting since an algebraic notation for the sine, cosine, tangent, etc., was available at the time. It was a calculus for trigonometric functions (and the notion of function itself) that was lacking. That is to say, solutions were not expressed algebraically in terms of logarithms, exponentials, or trigonometric or hyperbolic functions, but were obtained by mechanical construction – a mechanism allowing the solution curve to be drawn. One might consider as an example Leibniz's fundamental article, in which the Leipzig philosopher presents a machine for graphic integration capable of solving the so-called "inverse problem of tangents."[20]

The fact that Leibniz, champion of the power of symbolic reasoning to free the mind from the burden of geometry and mechanics, is thinking about a general approach to integration in terms of a mechanical tracing device is extremely interesting. It tells us that his pronouncements in favor of algebra and calculus did not obliterate the notion, which we encountered in the work of Descartes above, that a solution is provided by a construction, not by a formula. The same interest in providing constructions as solutions to differential equations is evident in papers by Johann Bernoulli and Varignon, and until the work of Vincenzo Riccati.[21] These practitioners gave solutions of differential equations not in symbolic terms (i.e., as the equation of the general integral), but as geometric or mechanical constructions of a class of curves, each of which has a tangent expressed by the differential equation. (To be more precise, the ratio between the differential of the ordinate and the differential of the abscissa, dy/dx, measures the slope of the tangent.)

The *Lex Homogeneorum* in the Leibnizian New Analysis

The magnitudes of interest for the early practitioners of the Leibnizian calculus were continuous magnitudes, such as distances, angles, surfaces, times, velocities, and forces. Still in the early eighteenth century, numbers were considered inadequate for representing these magnitudes. Indeed,

[19] Katz (1987). [20] Leibniz (1693).

[21] Solutions of differential equations as geometrical and mechanical constructions can be found, for example, in Bernoulli (1710); Varignon (1710). For an excellent discussion of the integration of differential equations by what was called "tractional motion," especially in the work of Vincenzo Riccati, see Tournès (2009).

continuous magnitudes were thought not to be measured by numbers because of the problem of incommensurable ratios, which cannot be expressed by a quotient of two integers, or, as we would say nowadays, by a rational number. For instance, the ratio between the circumference of a circle and its diameter was considered as geometrically well defined, but its numerical value is an infinite decimal expansion in which the decimal digits continue without a recurring period – an irrational number. The classic Euclidean theory of proportions, rather than the algebraic theory of equations, was thus preferred when the aim was to deal rigorously with relationships between continuous magnitudes. Consequently, the Euclidean theory was still highly valued.

Partly for this reason, at the turn of the eighteenth century, the symbols in the differential equations of Leibniz and the early Leibnizians, and in the Newtonian fluxional equations, were understood as standing for line segments, not functions. That is, the symbols stood for ordinates and abscissae, which, in turn, generated the curves representing functional relationships between variable continuous magnitudes and their infinitesimal increments.[22] The plane curves themselves, rather than the functions, were the main objects of calculus.

More precisely, curves occurred in three contexts. First, curves were of intrinsic interest for the geometer (for example, determining the properties of the cycloid elicited great interest in the middle of the seventeenth century). Second, curves, as noted above, were of interest as construction tools. Third, curves allowed representations of functional relationships between continuous magnitudes (as in the medieval "latitude of forms").[23] This was of enormous importance in natural philosophy. Today, we would say that the mathematized natural philosophers of the Scientific Revolution employed "graphs." Galileo, with his triangle representing the variation of speed in relation to time in free fall, had made these graphs famous. Mathematicians such as Evangelista Torricelli, Huygens, Isaac Barrow, Newton, Leibniz, and Varignon also represented the functional dependence of continuous magnitudes, such as times, spaces, speeds, and forces, in terms of graphs – plane curves whereby, for example, the abscissa represents a distance and the ordinate the intensity of force, or the abscissa the time and the ordinate the

[22] Bos (1974). Bos deals also with Leibniz's "transcendental law of homogeneity" (1974, 33–34).

[23] In fourteenth-century Paris, graphical representations were developed that allowed the visualization of the variation of continuous magnitudes, such as time and speed, related one to another by what, anachronistically, one would call a functional dependence. See Sylla (1973).

speed.[24] Varignon was particularly influential in promoting the systematic use of these graphs, as can be seen in a paper published in 1700, where a single figure graphs the various combinations of functional dependency among parameters of motion.[25] So, in Figure 15.5,

> The "curve of times" *TD* represents time as a function of distance; the "curves of speeds" *VB* and *VK*, the velocity as functions of distance and time respectively; and the "curves of force" *FM*, *FN*, and *FO*, the force as functions of distance, time, and velocity, respectively.[26]

Thus, for example, once a graph representing the variation of displacement s as a function of time t is given, the ratio between infinitesimal segments ds/dt defines the ordinate v (actually the ratio between v and a unit segment) of another graph (the "curve of speeds") representing the variation of speed as a function of time.

The fact that the symbols occurring in the equations stood for finite and infinitesimal line segments prompted the mathematicians who were using them to pay due attention to geometrical homogeneity. Thus, the *lex homogeneorum* was important for Leibniz and his acolytes and was often referred to in their works and correspondence when they dealt with differential

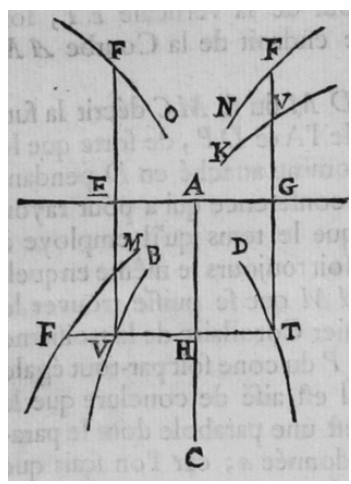

Figure 15.5. Varignon's graphs of functional dependencies among parameters of motion
Source: Varignon (1700, 22)

[24] Roche (1998, 87). [25] Varignon (1700, 22). [26] Mahoney (1998, 747).

equations. Just to give one example, in 1710, Johann Bernoulli wrote an expression of what nowadays we identify as conservation of mechanical energy for central forces:

$$\int \phi dx = ab - vv \tag{3}$$

where ϕ is the strength of a central force, x is the radial distance from the center of force, v is the speed, and a and b are two arbitrary constants of integration. Today, this equation, which appears in our textbooks and is taught to our students, would not be written with the constant of integration as the product of two constants, ab, but simply as one constant.[27] Yet, for Bernoulli, it is important to have a – from our modern point of view – superfluous constant because it is precisely this constant, which has the dimensions of a length, that guarantees the equation's geometric homogeneity, as Bernoulli explicitly states. In fact, Bernoulli conceives the intensity of force ϕ and the speed v as represented by line segments: they are the ordinates of graphs, as Varignon had detailed. The integral of ϕ times dx thus measures a surface; that is, the surface under the graph of ϕ as a function of x. In other words, ϕdx is a rectangle with finite height ϕ and infinitesimal breadth dx. The integral $\int \phi\, dx$ is therefore a surface made up of infinitely many infinitesimal rectangles, and it must therefore be made equal to a geometrically homogeneous magnitude – another surface. This requirement is satisfied in equation (3) by the fact that both ab and vv are two-dimensional. Geometrically, they are products of two line segments.

This practice of writing equations in the differential and integral calculi in accordance with a constraint on homogeneity was predominant in the early modern period. Until the middle of the eighteenth century, cases in which differential equations were written as non-homogeneous were rare. A review of all the differential equations published during the period in the *Acta eruditorum* or *Mémoires de l'Académie des sciences*, or appearing in the correspondence between Leibniz, Johann Bernoulli, and Varignon, would reveal how attentive these mathematicians were in following the *lex homogeneorum*; and the exceptions are an interesting subject of study. In the writings of Leibniz and the early practitioners of the calculus, especially in private correspondence and calculations, it is possible to find some occurrences of inhomogeneous differential equations.[28] The availability of a choice between

[27] As Bernoulli was aware, in equation (3) a factor of two is missing (Bernoulli 1710, 525).
[28] I thank Viktor Blåsjö for drawing my attention to this very important point.

two ways of writing differential equations makes all the more interesting that Leibniz and his acolytes – after discussing at length what is, for us, a non-problem – in so many instances deliberately refrained from expressing inhomogeneous equations. One often finds them correcting an equation by apologetically multiplying a term by the necessary constant "in order to guarantee homogeneity." The fact that Leibniz, Johann Bernoulli, Varignon, and even the young Euler used additional constants to guarantee the homogeneity of differential equations is notable, since it was clear from Descartes's *Géométrie* how homogeneity could be dispensed with.[29] This mode of expression reveals that, for them, the symbols occurring in differential equations had geometrical meaning.

Another advantage in constraining differential equations to geometrical homogeneity was that infinitesimal magnitudes could be added or subtracted in a meaningful way. This needs some explanation, since it is relevant to the handling of differential equations we have reviewed above. In the manuscript dedicated to the *mathesis universalis* already quoted,[30] Leibniz considers the handling of differentials. Let us take, for example, the formula

$$dx + d^2x + dxdx \tag{4}$$

What does this mean? If x represents a line segment, dx and d^2x are two infinitesimal lines (the former first-order, the latter second-order). Yet, $dxdx$ represents the infinitesimal square with side dx, and cannot be added to a line segment. Instead, as Leibniz argues, not only the algebraist, but also the practitioner of the calculus must observe the *lex homogeneorum*.[31] Accordingly, the proper expression is obtained by multiplying by a constant a in such a way that all terms are homogeneous:

$$adx + ad^2x + dxdx \tag{5}$$

In formula (5) we have a sum of three surfaces, a perfectly meaningful geometrical expression.

For Leibniz, and for his immediate followers, differentials always had a geometric meaning. They did not understand dy/dx as the derivative d/dx of a function $y(x)$, but rather as a ratio between two infinitesimal

[29] Once again, we have to pay attention to present-day terminology. The definition of a "homogeneous differential equation" today has nothing to do with the "homogeneity" in differential equations discussed by Leibniz and his acolytes, such as Varignon and the brothers Bernoulli. For the present-day definitions, see, for example, Simmons (1972, 35–38, 74).

[30] Leibniz (1962c, 66). [31] See also Leibniz (2018, 146).

magnitudes, dy and dx. Their differential equations were thus "equations relating differentials" in which, for the reasons discussed above, the constraint of geometrical homogeneity was important. But this means that the geometrical rules of Euclidean proportion were still operative, even in the seemingly modern algebraic analyses.

Toward Eighteenth-Century Analysis

Slowly things began to change, however. The theory of proportions, notwithstanding its rigor, is subject to serious limitations. When magnitudes are functionally related in a way that has some degree of complexity – for example, magnitudes related by fractional exponents – one is almost compelled to use algebraic symbolism. Thus, the early practitioners of the differential and fluxional calculi began writing differential and fluxional equations, rather than proportions, in order to deal with complex situations, such as motion in resisting media. Most notably, when functional relationships are expressed by transcendental functions, proportion theory was impossibly cumbersome. A new algebraic symbolism to handle trigonometric, logarithmic, and hyperbolic functions began to develop. This slow process was under way even in the middle of the eighteenth century.[32] In the 1720s and 1730s, especially in the Basle school, it seems homogeneity was becoming a less compelling requirement.

One preliminary step was Jacob and Johann Bernoulli's investigation[33] of the celebrated equation,

$$ady + ypdx + by^n qdx = 0 \tag{6}$$

This equation has a notable degree of generality, because the letters y, p, and q stand for ordinates that can be related to the abscissa x in any way. (We would say they are arbitrary functions $p(x)$ and $q(x)$, and for any choice of $p(x)$ and $q(x)$, one has to find $y(x)$.) Equation (6) thus encompasses a class of equations, thanks to the generality of the symbols y, p, and q. But this same generality also begins to weaken the constraints of geometrical dimensionality. For example, p could be a second-degree parabola and q a third-degree cubic. With this development, we see the beginning of a general, abstract notion of *function*. The pervasive use of this notion, which became one of the basic concepts of the calculus, made the requirement of homogeneity disappear around the middle of the eighteenth century.

[32] Katz (1987). [33] Bernoulli (1695, 553).

It is revealing to briefly consider the work of Euler. The "superfluous" constant added to make equations homogeneous, as discussed above, was still being used by Euler in his *Mechanica* (1736).[34] In the *Mechanica*, however, Euler consistently sought solutions as algebraic expressions, not as constructions. And Euler was soon to discard the constraint of homogeneity. In the second appendix of *Methodus inveniendi lineas curvas*,[35] where Euler derived the central force equations for the motion of a particle from the principle of least action, using the differential variational equations that he produced in the treatise itself, he was not concerned with homogeneity.[36] Furthermore, it was Euler who, in Chapter 4 of his fundamental treatise on the dynamics of rigid bodies, systematically defined units for a fundamental set of physical magnitudes: distance, time, velocity, mass, and force.[37] By doing so, he was systematizing a practice already in use in mixed mathematical disciplines, like astronomy and what we would today call engineering.[38] With this, mathematicians began writing differential equations with constants that had a physical dimension and were measured as numerical values of chosen units; for example, g for the acceleration of gravity in meters per second squared.

At this juncture, in the middle of the eighteenth century, Euler and other mathematicians of his generation, including Jean le Rond D'Alembert, were no longer referring the symbols in their equations, in Varignon's style, to graphs representing the relationships between continuous magnitudes. That is, they were not referring the symbols to geometric curves. Instead, they were deploying the algebraic concept of function.[39] Their differential equations were now understood in a more algebraic way and they consequently sought to integrate them in terms of classes of functions (as general integrals).

As noted above, the mid-eighteenth-century shift toward algebra was mostly due to the fact that the problems tackled became increasingly unsuitable to representation by geometrical or mechanical constructions. Furthermore, by the middle of the eighteenth century, the concepts of multi-variable function and

[34] See, for example, Euler (1736, vol. 1, ch. 3, prop. 25, §193).
[35] Euler (1744, 309–320).
[36] I thank Craig Fraser for noting this.
[37] Euler (1765, 71). For a discussion, see Roche (1998, 132–136).
[38] Engineers and astronomers needed to derive numerical values from their equations. For them, for example, it was not enough to say that in free fall, the spaces are proportional to the squares of the times. They wanted to know how many meters are covered in a given time measured in seconds. Therefore, they defined units of measure and interpreted equations numerically, typically in defining a numerical value for the gravitational acceleration. One of the first examples is Saulmon (or Saumon) (1715, 114). See Roche (1998, 129, 131–132).
[39] I thank Christian Gilain for noting this.

of partial derivative were introduced in order to mathematize continuum mechanics (the mechanics of rigid, fluid, and elastic bodies). In these fields, searching for solutions of differential equations in terms of constructions was an almost hopeless task. Euler soon abandoned any interest in it, and gave prominence of place to the notion of function, even in his didactic treatises, such as the *Introductio*.[40] The habit of requiring ordinary differential equations to be geometrically homogeneous faded away, too, and very few up-to-date mathematicians used the proportion theory that characterized Huygens's *Horologium* and Newton's *Principia*, two works that were much admired, but whose mathematical language was considered obsolete.

Conclusion

With the advent of the Cartesian "common analysis" and the Leibnizian "new analysis," seventeenth-century European mathematics underwent a profound advancement. This progress made the Scientific Revolution possible. Specifically, it allowed the solution of problems in fields such as cartography, ballistics, and astronomy, which had been foremost in the agenda of mathematicians and natural philosophers since the mid-sixteenth century. Yet, we should avoid reading this development as the abandonment of geometry in favor of algebra, since solutions were still sought as constructions and equations were still written in accordance with the *lex homogeneorum*. This methodological shift occurred later, in the middle of the eighteenth century. For the mathematicians of the Scientific Revolution, Pappus's canon of problem construction retained all of its importance. For the historian of philosophy and science, it is of some consequence to realize that, in their mathematical practice, even an advocate of the *mathesis universalis*, such as Descartes, and a proponent of the *characteristica*, such as Leibniz, highly valued the *geometrical* import of their symbolism.[41]

[40] Euler (1748, 2:6).

[41] For helpful comments, I thank Christian Gilain, Craig Fraser, Viktor Blåsjö, Marco Panza, Sébastien Maronne, David Rabouin, Nadine de Courtenay, Massimo Galuzzi, Michel Blay, Luigi Pepe, and the participants (most notably Sophie Roux, Jonathan Regier, Dana Jalobeanu, David Marshall Miller, Andrew Janiak, Brian Hepburn, Marius Stan, Sandro Caparrini, and Paola Basso) in the workshops held in Bergamo October 5–7, 2017, and at Iowa State University July 26–28, 2018. This research was funded by the "Piero Martinetti" Department of Philosophy of the University of Milan as part of the Project "Departments of Excellence 2018–2022" awarded by the Ministry of Education, University and Research (MIUR).

16

Mechanics in Newton's Wake

BRIAN HEPBURN AND ZVI BIENER

Introduction: Received and New Historiographies

In his influential *Die Mechanik in ihrer Entwickelung* (Mechanics and its evolution) (1883), Ernst Mach asserted,

> The principles of Newton suffice by themselves, without the introduction of any new laws, to explore thoroughly every mechanical phenomenon practically occurring, whether it belongs to statics or to dynamics. If difficulties arise in any such considerations, they are invariably of a mathematical, or formal, character, and in no respect concerned with questions of principle.[1]

This view of "Newtonian" or "classical" mechanics had become orthodoxy by the middle of the twentieth century. For example, A. Rupert Hall wrote that

> the refinements of the French mathematicians in no way modified the essential principles of mechanics, which were already fixed, although their more penetrating analysis enabled some new problems to be solved, and some old errors to be corrected.[2]

Implicit in both Mach's and Hall's assessments was the belief that Isaac Newton had reached the zenith of mathematical-mechanical thinking in the long Scientific Revolution. As a corollary, both suggested that Newton's achievement went unchallenged by later generations and that the system of thinking attributed to him was preserved with only minor modifications. Well-known eighteenth-century developments in mathematical formalism were consequently portrayed as *merely* formal, with no significantly novel semantic content. The possibility that changes in the mathematical language of physics could alter the physics it expressed was given relatively little attention.

[1] Mach (1919, 256). [2] Hall (1956, 340).

The mid-twentieth century turn to externalism in the history and philosophy of science pushed the question even further into the background. Investigations of the internal logic of physical theories were increasingly abandoned in favor of detailed studies of external determinants. In addition, historians rejected the traditional, physics-centered narrative of the Scientific Revolution. They replaced it with a set of more complicated views, but insofar as the development of mechanics in the eighteenth century was concerned, the (much needed) historiographical change simply preserved the received view in amber.[3]

At the same time, however, scattered historians of mathematics began looking intently at technical issues in eighteenth-century mathematics.[4] Their work has since taken root, and is now being expanded in multiple directions, with greater attention being paid both to the technical detail of post-Newtonian mathematization and to the conceptual work it accompanied. As a result, historians have found significant differences between Newtonian and post-Newtonian mathematization,[5] as well as significant differences in conceptual frameworks.[6] A more plausible thesis has emerged: The technical and formal developments of mechanics were conceptually rich. New mechanical concepts were embodied in new formalisms, and their articulation altered, sometimes subtly, often radically and foundationally, Newton's original framework. Thus, Sandro Caparrini and Craig Fraser note in a review article that

> the appearance in the eighteenth century of new physical principles and modes of description was organically linked to the mathematical elaboration of a coherent theory of mechanics.[7]

[3] For the intricate development of Scientific Revolution narratives, see Cohen (1994). For a sampling of the newer views, see Shapin (1996); Henry (2008b), as well as this volume.

[4] Among the best are Boyer (1959); Truesdell (1966); Bos (1993).

[5] E.g., Hankins (1970); Fraser (1989); Blay (1994); Guicciardini (1994); Fraser (1997); Guicciardini (1999).

[6] Thackray (1970); Blay (1994); Blay (1998); Boudri (2002); Maglo (2003); Ahnert (2004). We cannot address the question of whether the changes being described in the secondary literature mark the eighteenth century as "revolutionary" or not, in a Kuhnian sense (Kuhn 1970). We believe, however, that revolution and continuity are most appropriately ascribed to small-scale changes rather than to large historical periods that are virtually always comprised of patterns of small-scale revolution and continuity. For example, the increasingly refined articulation in the eighteenth, nineteenth, and twentieth centuries of the positions and masses of solar-system bodies is clearly continuous with the research program developed by Newton (Smith 2014). This development may best be viewed through the Kuhnian notion of "paradigm articulation" (e.g., Kuhn 1970, 23). However, many of the changes in dynamical theory for which this refined articulation provided evidence (some of which are discussed below) are more revolutionary in character, and were presented as such at the time.

[7] Caparrini and Fraser (2013, 359).

The historiographical challenge is now to construct coherent narratives that articulate more precisely this organic link and its evolution. In this chapter, we hope to contribute to these narratives.

In keeping with a theme of this volume, our aim is to describe the emergence of the new discipline of analytical mechanics as a response to specific problems, both technical and conceptual, found in the lacunae of Newton's original framework. We also describe it as a profound, if partial, departure from what came to be known as Newtonian natural philosophy. The transition from seventeenth-century natural philosophy to eighteenth-century analytic mechanics thus represents an inflection point in the history of science.

Analytical Mechanics, the Mechanical Philosophy, Transformation, and Continuity

But what changed? At the end of the inflection we trace, Joseph-Louis Lagrange declared mechanics to be a branch of analysis. He claimed his *Mécanique analytique* (1788) contained no geometrical or mechanical reasoning. And, in fact, other than historical summaries that prefaced each of the work's two major sections, it contained little or no reasoning of the kind Lagrange's predecessors would have recognized as mechanical – no discussions of machines, mechanical analogies, the fundamental properties of bodies, or, especially, causes. Yet *Mécanique analytique* is full of what, today, we call "mechanics," and which we recognize as physics. (The work presents much of what current textbooks call "Lagrangian mechanics.") Newton's *Principia* was criticized upon publication as "merely" a work of *mathematique*, not *physique*,[8] primarily for not identifying true causes. A century later, receptivity to the idea of a work of natural philosophy that was almost entirely mathematical had changed.[9]

The sea change in mechanics was brought about by three more specific developments: the adoption of the analytic calculus, a growing technical independence from Newton's *Principia* (including its three laws), and the subsequent explosion of solved problems. The three developments are linked in the following ways. The new formalism, the analytic calculus, afforded increased uniformity and intelligibility to the presentation of problems

[8] In a review (probably by Pierre-Sylvain Régis) in the *Journal des sçavans* (August 2, 1688). See Koyré (1965, 115).

[9] For the ups and downs of mathematics in physical theory in the seventeenth and eighteenth centuries, see Mancosu (1996); Warwick (2003); Guicciardini (2013).

compared to Newton's geometrical presentation. Problems were solved by seeking functional relations that remained the same despite the complex changes that might be involved in variable forces acting on moving bodies. These fixed functional relations led to the discovery of higher-order invariant quantities, ones that measured parameters at levels above the observable changes themselves. They also led to the discovery of higher-order equilibrium conditions, e.g., those embodied in minimization techniques or Lagrange's "general formula of the movement of any system." In this way, the analytic formalism allowed all first- and higher-order changes (changes of changes, or changes of combinations of quantities) to be represented and treated in uniform ways. This was a profound advance, as the earlier geometric formalism required different representations, and thus different solution methods, for each type of physical problem.

In many ways, the new mechanics of the eighteenth century stayed true to, but also expanded and completed, a project begun in the middle of the seventeenth century. In *Some Specimens of an Attempt to Make Chymical Experiments Useful to Illustrate the Notions of the Corpuscular Philosophy* (1661), Robert Boyle conceptualized the attempt to "deduc[e] all Phenomena of Nature from Matter and Local Motion" as the "mechanical" philosophy.[10] In so doing, he gathered together a host of competing philosophies that nevertheless shared a broadly anti-Aristotelian and anti-magical approach to natural philosophy, characterized by explanatory minimalism and ontological reductionism. In various ways and to varying degrees, they rejected final causes and promoted only material and efficient causes in explanation. They also deflated various rich conceptions of matter and its qualities and endorsed instead a sparer view, on which matter is possessed of (and perhaps defined by) a truncated list of privileged affections.[11] The list differed from thinker to thinker, but included extension, magnitude, figure, motion, impenetrability, and hardness (sometimes it also included fundamental powers like resistance or even life).

Two features of this "original" mechanical philosophy are relevant for its later development. Both stem from the fact that mechanical philosophers regularly held that their approaches to natural explanations were more intelligible than those of non-mechanical competitors. First, increased intelligibility was often attributed to the use of mathematics, since mathematical

[10] Boyle makes the point even more famously in *Origine of Formes and Qualities* (1666). See Jalobeanu (2006); Garber (2013c).

[11] The literature here is vast. See, for example, Dobre (2011), Jalobeanu (2011), Jalobeanu (2013), and references therein.

principles were recognizably intersubjective, certain, and inferentially transparent.[12] In fact, mechanics itself was seen as a branch of mathematics, a so-called mixed mathematics.[13] Whether mechanics was *merely* a branch of mathematics was subject to debate, of course, with many practitioners arguing that their new sciences should also be considered as significant contributions to physics. Importantly, the same type of negotiation also took place in the eighteenth century, but often in the opposite direction. The presumption was that mechanics was physical, but many practitioners hoped to imbue it with the kind of absolute certainty and clarity more appropriate to pure mathematics.[14] The details of these debates would take us too far afield, but our point is that the status of mechanics as physics and/or mathematics was not a settled question in either the seventeenth or the eighteenth century.

Second, increased intelligibility was also attributed to the use of machine analogies in explanation. Analogies to machines and their visible, moving parts were touted for their contrast to explanations by obscure forms, occult qualities, and metaphysically-complex entities. The "structural explanations" embodied in these analogies were premised on the idea that manipulation of parts according to simple laws is uniquely understandable.[15] The analogies also suggested an inherently reductive program for natural investigation: to understand a whole, break it into parts, break each part into its constituents parts, break each part again, etc. Taking this reductionism to its logical conclusion suggested that physical explanations ultimately had to appeal to the very smallest parts of machines; that is, to the very smallest parts of matter. Mechanical philosophers were overwhelmingly committed to the idea that, at the fundamental level, all matter is essentially the same and follows the same laws. From Boyle's "catholick" matter, to the generic unity of René Descartes's matter, to the atomism Newton expressed in the *Opticks*, the uniformity of matter became a working, and often unquestioned, hypothesis.[16] In practice, however, higher-level categories – such as Descartes's three kinds of body, or Boyle's solids, liquors, and gasses – proved

[12] See the chapters by Roux (ch. 4) and Van Dyck (ch. 14) in this volume.
[13] See Mancosu (1996); Cormack, et al. (2017).
[14] E.g., Euler (1736, 10). Of course, the idea of imbuing mechanics with mathematical certainty and clarity was not new to the eighteenth century. What was new was the desire to do so given the presumption that mechanics, as rendered in the language of mathematics, was unproblematically physical. For earlier efforts, see, e.g., Garber (2002b), Babeș (2018), among many others.
[15] Anstey (2000b, 55), following McMullin (1978).
[16] Boyle (1772a, 15); Newton (1952, Query 31); Descartes (1982, II.10).

indispensable for many mechanical explanations. These explanations required the interaction of different types of bodies, as, for example, in aetherial accounts of gravity or mechanical explanations of cohesion.[17]

Mechanics in the eighteenth century inherited the idea that intelligible explanations were reductive and structural, as well as the tension between the theoretical commitment to fundamental homogeneity and the practical need for distinctions between types of bodies. Taking apart bodies was only the first step, as it were, in rendering natural phenomena intelligible.[18] The following step was putting bodies back together, in mathematically tractable ways. This is precisely what eighteenth-century mechanical philosophers had to do in order to solve problems dealing with bodies like strings, columns, membranes, gasses, or fluids. This was a broadly mechanico-philosophical endeavor, *not* a Newtonian one. Newton's principles were of little help, especially when compounded with their non-analytic presentation. As I. B. Cohen puts it,

> in the *Principia*, largely, problems are investigated which depend wholly on the masses, motions, and positions of bodies and the medium in which they move: problems which lend themselves to treatment in independence of the state (save for motion or rest) of a body, its internal constitution, and the kind of material or materials of which it may be made. The *Principia*, in other words, presents a kind of physics of mass abstracted from real bodies . . . .[19]

Although Cohen here contrasts *Principia* with the more ecumenical *Opticks*, he points to exactly those problems left open by Newton that became the launching point for analytical mechanics. Practitioners of the analytic calculus had to address problems requiring consideration of the internal mechanisms – as Cohen puts it, "states" – of various types of bodies, in both their dynamic (that is, variable) and invariant aspects. This was a task to which the analytic formalism was well suited, but which was all but intractable through geometric representation. In treating these problems, eighteenth-century mechanists echoed their seventeenth-century predecessors (albeit with unprecedented precision and mathematical sophistication) more than they echoed Newton. Or, more precisely, they did not echo the Newton of *Principia* – the Newton the Machian narrative claimed they merely completed – but the more traditionally mechanical Newton of the *Opticks*. This is not to say that eighteenth-century rational mechanists operated in the tradition of the *Opticks*. Rather, unlike their more Baconian

[17] See Glanvill (1661); Huygens (1950). [18] Dijksterhuis (1961). [19] Cohen (1966, 118).

counterparts in Britain, continental mechanists relied on mathematics over exploratory experimentation, and insisted on providing mechanics with a conceptually coherent and necessary foundation. Although the requirement of necessity seemed incompatible with an empirical grounding, it was nevertheless the "rational" mechanists who proved far more successful than their British counterparts in solving the open empirical problems of their time.

In fact, it is important to note that the demand for secure conceptual foundations in rational mechanics was *not* on par with the demand for secure metaphysical foundations in seventeenth-century rationalist, speculative philosophy. One of the deepest methodological debts owed to Newton by his eighteenth-century successors was an inversion of the traditional, pre-Newtonian ordering of knowledge-generating disciplines. Whereas metaphysics was often seen as a prolegomenon to physics (e.g., by Gottfried Wilhelm Leibniz and Descartes), Newton insisted that physics itself was a guide to metaphysics. For example, he wrote that

> [in the *Principia*] we are concerned only with sensible things and their parts, for it is in these things alone that the inductive argument has its place. The other things which cannot be perceived, but yet are hypothetically termed bodies by some people, these things are more properly treated of in metaphysics and hypothetical philosophy . . . . Philosophy begins with phenomena. Experimental philosophy consists in treating of such things. One must pass from experimental philosophy to the efficient and final causes of things and from all these to the nature of imperceptible things and finally to hypothetical [i.e., metaphysical] philosophy.[20]

This attitude animated Newton's most famous public proclamation – that he does not feign hypotheses. His claim was not simply that speculation is off-limits. Rather, it was that truths about fundamental ontological levels are not required for establishing truths about less fundamental levels; and, moreover, truths about these "higher" levels serve as constraints for discovering truths about the "lower" levels.[21]

We see the same attitude in, for example, Euler's insistence that

> as it is Metaphysics which is occupied in the investigation of the nature and the properties of bodies, the knowledge of the truths [of mechanics] can serve as a guide in these thorny investigations.[22]

[20] Draft materials for *Principia*, Book III, quoted in McGuire (1995, 116).
[21] See, e.g., Gaukroger (2014, 20); Smith (2012, 371). [22] Euler (1750, 324).

Crucially, Euler held that only mechanics in its analytic form – because of its increased intelligibility – could serve this role. But adopting the idea that metaphysics is informed by physics is perhaps the most crucial way in which eighteenth-century mechanical thinkers were fundamentally aligned with *Newtonians*. At the same time, and precisely because mechanics would now take only what it needed from metaphysics, a mechanics that represented the world in a fundamentally new way – through functions rather than curves – would require changes in metaphysical commitments. Along with reshaping mechanics through the articulation of the new mathematics, one might also look to the forms of conceptual clarification which accompanied that transformation. Concepts like force and mass and action were informed by adopting a mathematics of infinitesimals and of functions. The influence could go in both directions, resulting in new, mathematically motivated concepts, such as Jean le Rond D'Alembert's virtual work or Euler's understanding of inertia.[23]

Taken together, the inversion of the order of disciplines, the emphasis on the rational intelligibility of abstract quantities and functional relations, the reliance on formalism as a vehicle for this intelligibility, as well as the recognition that different bodies required different mathematical techniques, constituted the emergence of a new discipline, though one with clear roots in the previous century's mechanical philosophy. It was a discipline with its own methods and standards of problem solving, as well as a shared set of problems to which to apply them. This new discipline would foster a blurring of the distinction between mechanical and mathematical problem solving. Mechanical reasoning would become mathematical reasoning.

A common theme of some rejections of the standard narratives of the Scientific Revolution is that those narratives have overemphasized mathematization and mechanics, at the cost of underemphasizing broader cultural and conceptual developments.[24] Others, like Johann Christiaan Boudri, argue that the overemphasis on *mere* mathematization has underemphasized the important conceptual work done within mechanics itself.[25] Our narrative seeks to complement both of these literatures by stressing the importance of mathematical developments to the new concepts in mechanics and hence the influence of mechanics more broadly.

This narrative is programmatic, of course. We cannot hope to provide here the historical and technical detail the full story requires. We provide

[23] For D'Alembert's principle, see below. For Euler on inertia see Hepburn (2010) and Stan (ch. 21) in this volume.

[24] For instance, Osler (2000a); Zinsser (2005c); Hagengruber (2012).

[25] Boudri (2002).

scattered evidence, however, by providing a series of examples leading up to Lagrange's declaration that he has rendered mechanics a branch of analysis. To understand these, we first examine the set of problems eighteenth-century mechanics set out to solve.

Open Questions of the *Principia*

The fundamental successes of Books I and III of the *Principia* depend on treating only centripetal (or approximately centripetal), conservative forces.[26] The limitation allows Newton to employ his geometric devices built around a central point: the area law, the linear sagitta, and the "linear dynamics" solid.[27] Anachronistically put, the area law provides a conservation principle and a measure of time; the sagitta a geometric representation of the curvature of a trajectory (an indirect representation of the magnitude of force, but dependent on velocity); and the solid a geometric representation of the magnitude of a force at any point of a curve (after velocity and curvature are taken into account). These, coupled with Newton's method of first and last ratios, allow Newton to overcome a major challenge of mechanics; namely, to represent the complexity of a force whose direction and strength is changing because it is acting on a body whose motion is changing in consequence of the force itself. The method of first and last ratios allows Newton to represent such continuous changes by a series of similar, finite representations. From these, he can discover mathematical relations that remain invariant as the finite representation nears a continuous change; i.e., in the infinitesimal limit. These geometric devices, however, are valid representations only for conservative centripetal forces. (The method of first and last ratios itself is not limited in this way.)

In Book II of the *Principia*, Newton aims to solve a broader set of problems, ones that are not solely dependent on conservative centripetal forces. These are problems of fluid dynamics, of bodies moving through resistive media, and bodies constrained to various non-orbital trajectories. Losing the centripetal constraint dramatically affects the lengths Newton must go to, and the strategies he must employ, in order to solve these problems. For example, in

[26] Newton also replicates earlier Huygensian and Galilean results, which take gravitation to act in parallel lines. These cases can be subsumed under the treatment of centripetal force by assuming that the center of force is infinitely distant.

[27] See Lemmae and Propositions 1, 6. The solid in question is inversely proportional to the strength of a centripetal force. Brackenridge (1995, 7) calls the inverse quantity "the linear dynamics ratio."

order to construct the trajectory of a projectile moving in a resistive medium, Newton employs a hyperbola to represent the relations among speed, time, and distance. But how those quantities relate to the hyperbola varies according to the resistance force function; for instance, whether the resistance is proportional to velocity or velocity squared. That the hyperbola works at all follows from a particular geometrical relation to the exponential: the area between the hyperbola and one of its asymptotes grows exponentially as you approach the other asymptote. Thus, it is useful for problems in which quantities change in proportion to themselves. Newton puts this to ingenious use at Propositions I–IV, in Section I of Book II, for media where the resistance is directly proportional to velocity. But he then has to find another, equally ingenious way, in Section II, to use the hyperbola for the case of a resistance proportional to the square of the velocity.[28]

Euler took this as a sign that treating mechanics geometrically does not provide true understanding – one's ability to solve a problem should not be upended by a minor change to the problem. Both above problems are solved by Euler – using analysis – as part of the general problem of "media of whatever resistance."[29] Analysis, Euler thought, far better than geometry could provide the understanding needed to generalize solutions to problems in mechanics. As Euler would point out later, the chief difficulty of a general science of motion – anything worthy, in Euler's estimation, of the name *mechanics* – is that forces are applied to moving bodies in ever-changing relations of direction and magnitude. To solve such problems *in a generalized way* requires discovering some underlying and unchanging rules governing the interactions between force and body. Euler's insight was that these rules might be discovered in the natures of the various types of bodies.

In the next section we look briefly at some of the ways that this was done in the eighteenth century, telling the story, through some few examples, of the development of mechanical principles in tandem with developments in conceptions of physical quantities and formal developments. This part of our narrative is that the development of analytic mechanics was an evolution from a natural philosophy concerned more narrowly with the generation of motions by forces, to a physics concerned with representing invariant mechanical relations among physical quantities (especially in analogy with the balance or equilibrium relations). In this way, the need for a reduction to point masses to which Newton's laws could be applied was obviated. It was

[28] Newton (1687, 236). [29] Euler (1736, Prop. 105).

replaced, instead, by systems of bodies, linked by mechanical constraints expressed through functional relations. The evolution took place in an anti-reductionist way, from point masses "upwards" to a mechanics of integrated bodies.

New Programs in Mechanics

In 1736, ten years after the publication of the last edition of *Principia*, Euler described his view of the program by which the science of mechanics should proceed, based upon the composition of bodies. Mechanics, for Euler, is firstly to be contrasted with statics. Statics assumes an equilibrium of forces, so that there are no changes to the states of motion of the bodies involved. Mechanics, on the other hand, is the science of moving bodies. Forces are not at equilibrium, resulting in changes to the states of motion of the bodies. Of particular difficulty for the science of mechanics, then, is to calculate the effects of forces on moving bodies.

> In Statics, where all things are assumed to remain at rest, all *potentiae* [state-changing powers] are set up to perpetually keep their same directions. But in Mechanics, as a body perpetually comes to other positions, the directions in which the *potentiae* act on [the body] continually change. According to the different places of bodies or directions of *potentiae*, which will be parallel to one another or converging to a fixed point, or holding to some other rule, from that variety of *potentiae* in Mechanics, the discussion arises.[30]

The orientations between the forces, the bodies, the direction of their motions, and the resultant changes, are all continuously varying. Newton's laws dictate that the changes in the motions are proportional to, and in the direction of, the forces. But when those directions are continuously changing, the application of the laws would be continuously changing. Newton's laws simply cannot be applied to all but the simplest of problems, or those where some simplifying constraint or invariance can be found. The prime example of this are problems with a centripetal force, as discussed above.

On the face of it, then, the generalized problem seems intractable. Any continuously varying trajectory would require an infinite number of discrete applications of Newton's laws to correspond to the infinite number of directions in which the forces act on the body. This problem holds for the motion of points considered on their own, but is compounded for points aggregated into bodies. Bodies are systems of points, each interacting with

[30] Euler (1736, 41).

one another, adding another layer of complexity to the calculation of forces and effects.

Euler's proposed strategy was to work his way "up" through bodies of different natures, beginning with forces on free point masses (not limited to centripetal forces), then points constrained to motions on surfaces or moving in media, then rigid bodies, elastic solids, non-rigid bodies of several types, and, lastly, fluids.

> The diversity of bodies therefore will supply the primary division of our work. First indeed we shall consider infinitely small bodies . . . . Then we shall attack bodies of finite magnitude which are rigid . . . . Thirdly, we shall treat of flexible bodies. Fourthly, of those which admit extension and contraction. Fifthly, we shall subject to examination the motions of several separate bodies, some of which hinder [each other] from executing their motions as they attempt them. Sixthly, at last, the motion of fluids will have to be treated.[31]

The point of the program is that each kind of body represents a different set of constraints on the motion of the points of that body. What makes the latter bodies more difficult is that the constraints themselves change in more and more complicated ways. Rigid bodies have fixed constraints, fixed relations among the parts. Elastic bodies have changing relations, but the changes can be described in fixed ways. With extension and contraction, the constraints can change, and so can the rules by which the constraints change. Fluids are the most complex of all. Surprisingly, it is here that Euler has the most success, though hard won. (This can be contrasted with Newton's attempts at treating fluids in *Principia*, Book II.)

Several other strategies emerged to deal with the difficulties of change (and of changing change, and changing change of change, etc.). Commonly, these began with attempts to extend equilibrium in static cases to dynamic ones. Pierre Varignon, for instance, sought to reduce the understanding of all machines to a principle which could be ascribed to the lever. He explained, in the Preface to *Projet d'un nouvelle mechanique*, how he came across a remark of Descartes's that it seemed ridiculous to apply the principle of the lever to a pulley. Varignon demonstrated nevertheless how one could first apply the principle to a body on an inclined plane, and then to the pulley. The link was to consider the equilibrium of forces.[32] Although Varignon was one of the prominent figures to work on translating *Principia* into analytic form, his work was overshadowed by that of Jakob Hermann and Johann Bernoulli.[33]

[31] Euler (1736, 8–9). [32] Varignon (1687). [33] See Guicciardini (1999, §8.4).

His greater contribution was through his systematization of equilibrium conditions in statics and the simple machines.

D'Alembert was even better known for extending the concept of equilibrium to dynamic problems. D'Alembert's principle, as it would come to be known, requires introducing fictitious forces which, were they present, would result in the equilibrium of the body under consideration. Non-static quantities, in other words, could be treated through an invariant, equilibrium relation. This approach allowed for the successful solving of problems of vibrating strings, and of fluid mechanics, including bodies moving in a frictional medium.

In D'Alembert's 1743 treatise on solid bodies, he claimed his Laws were founded on metaphysical principles independent of experience. When the principles are metaphysical one can "determine exactly those of the Principles which must serve to found the others."[34] Moreover, "[t]he Laws of the Mechanics of ordinary Bodies . . . can be reduced to three; known as the force of inertia, the composition of movements, and the equilibrium of two equal masses moved in contrary directions by two equal virtual velocities."[35]

However, when it came to fluids, D'Alembert could no longer found the project on metaphysical principles independent of experience. He expressed the intent to do so in his 1744 *Traité de l'équilibre et du mouvement des fluides*:

> In the *Traité de Dynamique* . . . I aimed to reduce to the smallest number possible the Laws of the equilibrium and movement of solid Bodies; I have tried to do here the same thing for Fluids.[36]

And D'Alembert argues that, since fluids are composed of material components, all three of his Laws of Mechanics for ordinary bodies will apply to them. However, since "we do not know the form and the disposition of fluid particles," the third law, concerning individual parts and their virtual velocities, is of no use in discovering the equilibrium conditions of fluids. We must appeal to experience: "Experience alone can instruct us on the fundamental Laws of Hydrostatics."[37]

The "experiences" D'Alembert describes are what we might call phenomena. But they are also very close to principles in and of themselves. As an example, D'Alembert asks us to consider a cylindrical tube bent in a sort of U-shape, and containing a liquid. That is, there are two vertical parts joined

[34] D'Alembert (1743, vi).
[35] D'Alembert (1743, viii).
[36] D'Alembert (1744, vi).
[37] D'Alembert (1744, ix).

by a horizontal one, with an opening at the top of each side. The fluid in the tube will only be at equilibrium when the two vertical columns of water are of equal height. At equilibrium, then, the fluid everywhere in the horizontal part will be subjected to equal pressures in both directions – the more general property being that fluids are pressed, and press, equally in all directions.

> This general property, shown by an Experience so simple, is the foundation of all that one can demonstrate on the equilibrium of Fluids.[38]

The point is that the foundations of any domain of mechanics, as D'Alembert sees it, are principles (laws) about equilibrium conditions.[39] In simple domains you can read those principles right off your metaphysics. For more complicated bodies, like fluids, you have to look to experiences. But the required experiences will be those that tell you about the equilibrium conditions for bodies of that type. You have to observe how they behave *as systems* because of their complexity. The right experiences will provide you with equilibrium principles on which to build the rest of the science.

Even within the domain of fluids we see D'Alembert proceeding through a series of bodies of ever-increasing complexity. Book I, on the equilibrium of fluids, proceeds by chapters through fluids only under their own weight, then under pressure from another weight but in a constant direction, fluids in vessels, with floating solids, in various shapes of tubes, different combinations of these conditions, and finally up to elastic fluids. Then, in Book II, D'Alembert does essentially the same thing all over again but for fluid systems *not* at equilibrium. In a remark at the beginning of Book II, D'Alembert makes explicit the connection between the first two books.

> It is evident that this Theorem is nothing other than the application of our general Principle of Dynamics to the movement of Fluids. As all the Laws of the movement of solid bodies . . . have been reduced by this Principle to the equilibrium of these same bodies, the Laws of the movement of Fluids can also be reduced in the same way to the Law of Equilibrium of Fluids.[40]

"Our" general Principle of Dynamics to which D'Alembert is referring is roughly the one we now call D'Alembert's principle, alluded to earlier.

[38] D'Alembert (1744, x).

[39] If D'Alembert has a principled distinction between the terms 'Principle' (*Principe*) and 'Law' (*Loi*), we cannot detect it. For example, he says that the "famous Law of Mechanics, called the conservation of living forces" is a "Principle today recognized as true by all Mechanicians" (D'Alembert 1744, xvi).

[40] D'Alembert (1744, 70–71).

Lagrange introduced yet another invariant relation among changing quantities, his "general formula for the movement of any system of bodies," in 1788. Lagrange's treatise has the same two sections that D'Alembert employed in his two treatises on solid bodies and fluids. That is, first one treats the equilibrium conditions and cases, and then the non-equilibrium cases. Lagrange called this statics and then dynamics, rather than equilibrium and then movement. The books are united by a common mechanical principle, functionally represented, but given in two versions in the two sections. First, at the introduction of the statics section, where the formula was a true equilibrium condition, the principle contained no accelerations, and only virtual velocities which sum to zero. At the beginning of the dynamics section, the same principle was given in the more general form to include accelerations, and given as a sum or integration over all the bodies of the system. The accelerations were, of course, implicit in the static version of the principle, except each acceleration was individually equal to zero. The virtual velocities, on the other hand, only equated to zero when summed. "From which," Lagrange comments, "one sees that the laws of movement of a system are the same as those of its equilibrium, in simply adding the new accelerative forces."[41]

There is one more apparent and significant difference between the static and dynamic versions of the "general formula." Namely, the dynamic version requires what Lagrange calls *variations*, rather than differentials, as in the equilibrium case. Variations are required because Lagrange is considering *systems* of bodies in which the bodies constrain each other's motions. To find the actual moments of the forces in the dynamic case, one multiplies the forces for each direction by the variations for that direction. These are the actual mechanical *moments*, because the variations are the actual mechanical *movements*. But the analytical, functional representation makes clear the connection between the equilibrium and dynamic cases.

> For the general formula of equilibrium consisted in that the sum of the *moments* of all the forces of the system must be null . . . thus one has the formula sought after by equaling to zero the sum of all the quantities relative to each of the bodies of the proposed system.[42]

These two cases illustrate how invariant relations could be used to render problems in mechanics intelligible and tractable, even while expanding the set of solvable problems to more and more general kinds of bodies,

[41] Lagrange (1788, 193). [42] Lagrange (1788, 195).

understood as mechanical systems. The analytic approach made it easier, even straightforward, to formulate relations among continuously varying quantities, but also the rates of change of those quantities, the rates of change of their rates of change, etc., and combinations of those with other quantities. Euler remarked explicitly on the importance of analysis for setting up, solving, and understanding problems in a now well-known and oft-quoted passage from the Preface to his *Mechanica*:

> But in all writings which are written without analysis it happens most in Mechanics that the reader, although convinced of the truth of those things which are put forward, nevertheless does not achieve clear and distinct knowledge of them, and so can barely solve the same questions by his own devices when they are altered even a little, unless he engages in analysis and explicates the same propositions using an analytical method. This often happened to me when I began to read through Newton's *Principia* and Hermann's *Phoronomia*: although I seemed to myself to have understood the solutions to many problems, still I could not solve other problems that differed even a little.[43]

A new discipline rose up around this strategy of searching for *unchanging* relationships, analytically expressed, which would allow mechanics to be extended beyond problems in celestial dynamics.[44] Invariant relationships were needed, like the area law, but for situations involving forces, motions, and constraints which were changing in complex ways – situations which arose outside central body problems, and for bodies whose internal structure could not be reduced to a mathematical point mass. These unchanging relationships were sought either in higher orders of the changes (i.e., dynamic equilibrium conditions such as D'Alembert's or Lagrange's) or in looking for conserved quantities, such as *vis viva* or action.[45]

Mechanics Has Become Analysis

In the *Avertissement* to the *Mécanique analytique*, Lagrange remarks that "Those who love Analysis, will be happy to see Mechanics become a new branch, and thank me for having extended the domain."[46] Given the emphasis on analysis, it is not shocking that the work contains no figures, nor do the methods require constructions or geometrical reasoning (as he

[43] Euler (1736, 9). [44] Hepburn (2010); Caparrini and Fraser (2013).
[45] On least action see, e.g., Lagrange (1788, 188–189). [46] Lagrange (1788, ii).

remarks in the same paragraph). But very puzzling is the rest of the sentence, denying that any *mechanical* reasoning is required. Analytic mechanics, for Lagrange, is mechanics without mechanical reasoning. What analytic mechanics does require are "only algebraic operations, subject to a regular and uniform procedure [*marche*]."[47]

Two important points relevant to our narrative can be highlighted here. Firstly, far from Lagrange being solely responsible for this new arrangement, and despite his proclamation, he is a signal of the inflection that had been taking place: the transformation of mechanical problems from problems of natural philosophy (including metaphysics) into problems of mathematics.[48] D'Alembert had earlier remarked on the important connection between mathematical and mechanical advance:

> Although the Physics of the Ancients was neither so unreasonable, nor so limited as some modern philosophers think or say, it appears, however, that they were not well versed in the sciences that we call Physico-Mathématiques, and which consist in the application of calculation to phenomena of nature.[49]

Because of the ready availability of mathematical techniques and solutions, mechanics adopted the standards of success of the mathematical domain. This inflection and discipline-merging was not entirely one-sided. Mechanics became mathematics, but not purely. Mechanical intuitions like equilibrium and balance and symmetry were still a part of the discovery and justifications of principles.

The new analytic mechanics was conceptually unified, but unified in a clear formalism, technique, and syntax, as well – far less obscure in its explanations and solutions than was Newton's *Principia*. Newton feigned no hypotheses, but nevertheless, or even for that very reason, there is much in his explanations we must simply take for granted: the *vis insita*, the strength and action at a distance of the gravitational attraction, the equivalence of inertial and gravitational mass, the law of equal and opposite action. These are powerful explanans, but they are not explained. Most magical of all is Newton's approach to the solving of problems. He seems to pull out of thin air the constraints and quantities he needs to make physical phenomena tractable. The linear dynamics solid, for instance, is not an obvious measure of centripetal force, and it is not clear how one arrives at a hyperbola to represent motion through

[47] Lagrange (1788). [48] See Stan in this volume (ch. 21). [49] D'Alembert (1752, vii).

a resistive media. The solutions were beyond impressive but hardly instructive.

In Lagrange, the mechanical reasoning seems less magical. We recognize it there on the page as parts of equations: functional relations. The equations look like machines. You can follow the moving parts, watch the quantities being manipulated. The equations express equilibrium interactions, with quantities, actions, activities, on both sides of the equation literally being equal to one another. In fact, Lagrange's preferred form of expressing his mechanical principles was with all factors on the left side of the equation and nothing but zero on the other. The solutions to many problems offered by Lagrange, or by Euler, remain standard in courses now called "classical mechanics."

Conclusion

We have attempted to sketch a narrative for understanding developments in eighteenth-century mechanics. Our narrative weaves together these elements: the role of the analytic formalism; the need for new principles to solve problems in mechanics for phenomena that do not involve centripetal forces, particularly kinds of bodies characterized by more complex internal interactions; and the functional conception and representation of mechanical phenomena. Our main conjecture is that the third point links the first two. It is an affordance of the analytic formalism that it allows for the functional representation of the interactions among the parts of complex systems. This was because not only could you straightforwardly represent changes to system quantities in the analytic formalism through differentials, interactions among those quantities could then be straightforwardly represented as functions once the relevant dynamic equilibrium principles were grasped. Our narrative sketch describes developments in mechanics as largely a search for those equilibrium principles through investigating the physical conditions that characterize more complex types of bodies, and physical systems more generally. By making mechanics a part of analytic mathematics in this way, mechanical understanding was greatly advanced at the same time as mathematical problem-solving techniques of the new formalism were exploding. The result was a new discipline, a hybrid of mathematical and mechanical reasoning, treating bodies as systems – as machines – reduced to simple mechanisms, rather than points.

It is true that mechanics in the eighteenth century was mechanics in a post-Newton world. This was a period in which the Continent became

enamored with the Enlightenment image of Newton as a paragon mathematical natural philosopher. This image was inescapably felt through its influence on the spirit of the age. But the real work done in mechanics in the eighteenth century, when attended to closely, does not usually begin with Newton, nor follow Newton. It was merely done in Newton's wake.

PART III

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PROBLEMS AND CONTROVERSIES

17

Galileo's *Sidereus Nuncius* and Its Reception

DAVID MARSHALL MILLER

In early 1609, Galileo Galilei heard reports of an optical device recently invented in the Dutch Republic that could magnify objects two or three times. Already familiar with lenses, Galileo correctly guessed the basic construction of the instrument, and was soon able to produce a version that improved magnification by an order of magnitude. By the fall, Galileo was using his *perspicillum* to observe the heavens, where he made several remarkable discoveries: minute features of the illumination of the Moon's face suggesting it has mountains; that the Milky Way and nebulae are not continuous and cloud-like, but congeries of discrete stars; and – most surprising of all – that Jupiter is accompanied by four moons. Jealous of his priority, Galileo rushed his discoveries into print, and *Sidereus nuncius* emerged from the press in March 1610.

Insofar as the Scientific Revolution is conceived as a reconstitution of the theoretical understanding of the natural world, the *Sidereus nuncius* is justifiably thought of as one of its seminal texts. Galileo's observations upended the Aristotelian–Ptolemaic geocentric cosmology few had ever doubted; i.e., the heavens consist of perfect and unchanging spheres rotating uniformly, while the mutable terrestrial realm lies at the center, within the sphere of the Moon. Even if Galileo left them to the reader, the observations reported in *Sidereus nuncius*, and Galileo's interpretations of them, had clear Copernican implications. If celestial objects like the Moon and Jupiter had terrestrial features like mountains and moons, then it was more likely that the Earth, too, was a celestial object, orbiting the Sun.

Yet the Scientific Revolution was also a reorganization of the intellectual disciplines by which the natural world was studied.[1] The *Sidereus nuncius* was pivotal in this process of disciplinary admixture, as well. In particular,

[1] See Biagioli (1989); Garber (2010).

Galileo's treatise combined methodologies and topics that traditionally had been assigned to distinct disciplines – mathematics and natural philosophy. However, in the period leading up to Galileo, the disciplinary bounds between them had been weakened by increasing social interaction and controversies. *Sidereus nuncius* broke them down altogether, applying the methodology of mathematics in the domain of natural philosophy by asserting the probative value of observations for and applying quantitative reasoning to natural explanations. Moreover, the excitement surrounding *Sidereus nuncius* led to its lasting cultural significance.[2] The book generated a lively and persistent discourse framed by Galileo's assertion of mathematical empiricism and quantification, such that even those who disagreed with Galileo responded on those grounds. Thus, the book – and its reception – reconfigured mathematics and natural philosophy into a novel form of "scientific" discourse.

Disciplinary Bounds

As we shall see, Galileo's discipline-mixing was remarked upon by nearly every author who initially responded to the *Sidereus nuncius*. But if Galileo's readers agreed that his mixture of disciplines was significant, what, the modern scholar is led to ask, was its significance? If the distinctions between disciplines were accompanied by differences in their methods, what did the disruption of that order do to the foundations upon which scientific knowledge was established? This is the question upon which this chapter will focus, though to understand the methodological integration of "natural philosophy" and "mathematics," it is necessary to first consider, in very broad outline, the *status quo ante*. Thereafter, their eventual recombination will be addressed.

Traditionally, natural philosophy was a scholastic discipline, taught in the universities as preparation for higher learning in theology, law, or medicine, and aimed at certain knowledge – *scientia* – of natural phenomena. Its guiding texts were the Aristotelian *organon*, especially the *Posterior Analytics*, which explained how *scientia* is achieved once the causal order of nature has been rendered into deductive syllogisms derived from indubitable principles. The logical necessity of a "scientific demonstration" thereby recapitulates the causal necessity in the world. A successful natural philosophical account elucidates the intrinsic, necessary characteristics of a thing, showing why it is the way it is and "that it is not possible for this to be otherwise."[3]

[2] See Ostrow (1996); Booth and van Helden (2000); Reeves (2013); Reeves (2014); Bucciantini, et al. (2015).

[3] *Posterior Analytics* I.2 (71b10–12) (Aristotle 1984, 1:115).

This Aristotelian conception of the aim of natural philosophy limited the role of observation. Most importantly, the insistence on certainty and necessity entailed that scientific demonstrations proceed from necessary universals rather than contingent particulars. One could not be sure that what is true of the particular holds for all similar cases – features of one instance might be mere accidents peculiar to that individual. Hence, discrete experiences – i.e., observations, even in aggregate – could not provide grounds for theory acceptance or rejection.[4] It follows that one could neither construct an explanatory theory on the basis of observations, nor disprove a theoretical claim by offering any number of counterexamples. Instead, the elucidation of causal natures was drawn from already generalized experience of universal kinds – for example, by general experience of dogs, one comes to an understanding of "dog."[5] Natural philosophy is the subsequent demonstration of what necessarily follows from the universal concept thus acquired. No *scientia* can be gained by looking at Fido, and his unfortunate amputation and mange do not disprove the universal claim that dogs are furry and four-legged.[6]

Stemming from the same Aristotelian root is another limitation on the use of quantification. In the Scholastic framework, the causality relevant to natural philosophy is embedded in the universal nature of an object – in what it is to be the kind of thing that it is. It follows that causal explanations must refer to the intrinsic natures of the objects considered, and, conversely, one cannot apply scientific demonstrations outside their own proper domain. This is the prohibition against *metabasis*: the nature of one kind does not explain the nature of any other. Strictly speaking, a *scientia* of dogs would not apply to cats. This prohibition stands in the way of using mathematics in natural philosophy.[7] Though even Aristotle was willing to grant that mathematical demonstrations are certain and explanatory when considering their properties, mathematical objects are not *natural* objects. Within natural philosophy, then, the focus must be on the distinctive *qualities* of things. Their merely accidental *quantities* can have little explanatory import. A bronze globe might be round, but it is not a mathematical sphere. So,

[4] See *Posterior Analytics* I.31 (87b29–99a6) (Aristotle 1984, 1:144).
[5] Compiling observed particulars was the business of natural history, a distinct discipline. Universal concepts could also be obtained from authoritative texts. Hence the common reliance on authorities.
[6] Wallace (1974); Dear (1987, 145); Bayer (1997).
[7] *Posterior Analytics* I.7 (75a38–75b20) (Aristotle 1984, 1:122). See Capecchi (2018).

strictly speaking, the properties of the mathematical sphere cannot imply anything essential or causal about the physical globe.

On the other hand, as traditionally conceived, the mathematical disciplines operated outside both disciplinary bounds. First, mathematicians circumvented the bar against *metabasis* by means of *subordination*. In a subordinate science, some aspect of pure, Euclidean mathematics is combined with premises drawn from natural philosophy to create a "mixed" mathematical discipline.[8] In such sciences, mathematization generates knowledge of the phenomena, because the phenomena in question happen to be subject to quantitative description. For instance, observational astronomy is the geometry of circles and spheres to which is added the physical premise that planets move by rotating around a center. This mixed mathematics then enables calculation of planetary positions.

Second, the work of mathematical practitioners was responsive to observations. Many worked in practical fields, where predicting particular outcomes was paramount. For example, the astronomer's task was to devise an arrangement of the celestial motions that could successfully "save the phenomena" – i.e., predict celestial observations. A successful or failed prediction was therefore grounds to accept or reject a postulated planetary arrangement or *hypothesis*. Astronomers, that is, tested their hypotheses against what was observed.

Nevertheless, the traditional ordering of the sciences still constrained what could be mathematized and empirically tested. Astronomers, like other mathematicians, were indifferent to or even uninterested in speculations about the natures of celestial objects and the causes of their motions, since they simply inherited premises about such things from natural philosophy.[9] The "higher" discipline dictated that the planets' aetherial natures caused them to rotate uniformly in circles, a motion subject to mathematical characterization; the astronomer had only to figure out the accidental parameters of the motions.[10] An observation might be grounds to reject the parametrical aspects of a hypothesis, but it could not refute a claim about the natures of celestial bodies or their motions. For example, Nicolaus Copernicus himself was moved to adopt geokineticism by the conviction

[8] McKirahan (1978); Mancosu (1996); Feldhay (1998). [9] See Laird (1986).

[10] This arrangement of the disciplines was ancient. See, e.g., Simplicius, writing in the sixth century: "For, in general, it is not for astronomers to know what is by nature at rest and what sorts of things are moved. Instead, by introducing hypotheses [i.e., geometric representations] of some things being stationary, others in motion, they investigate from which hypotheses the phenomena in the heavens will follow" (Bowen 2007, 344). See also Dreyer (1953, 131–132).

that the Ptolemaic planetary hypotheses "contradict the first principles of uniform motion" inherited from Aristotelian cosmology.[11] Likewise, Andreas Osiander's (in)famous *ad lectorem* noted that, since the astronomer "cannot in any way attain to the true causes, he will adopt whatever suppositions enable the motions to be computed correctly," leaving it to philosophers to judge the truth of those suppositions.[12] Viewed according to the traditional hierarchy of the sciences, then, novel observations like Galileo's could not prove the terrestrial nature of the Moon or the celestial nature of the Earth. Rather, they were simply yet-to-be-explained on the basis of the Moon's celestial nature and the Earth's terrestrial one, which were taken for granted within the astronomical discipline.[13]

Boundary Erosion

Such might have been the traditional view, but the boundaries that separated the disciplines were already eroding by Galileo's time. Within the sphere of natural philosophy, particular observations had begun to gain epistemic importance. Some of this derived from the inclusion of medicine as a higher faculty in medieval and Renaissance universities, joining theology and law as a subject to be studied after training in the arts and philosophy. Medical diagnosis required the discovery of unknown causes and natures from observation of particular cases, contrary to the prescriptions of Aristotelian method. Consequently, philosophers made efforts to develop demonstrative methods based upon individual observations, in order to lay a foundation for medical studies.[14]

In this vein, a consensus coalesced around what became known as the method of *regressus*, which originated in the fourteenth century, but became widely known as a standard procedure in natural philosophy by the end of the sixteenth, when it was epitomized in Jacopo Zabarella's *De regressu* (1578). *Regressus* combines three inferential steps. On the basis of observed effects, the *demonstratio quia* establishes the existence of some cause that is constantly

[11] Copernicus (1978, 4). See Miller (2014, ch. 2) and the chapter by Omodeo and Regier in this volume (ch. 13).

[12] Copernicus (1978, xvi).

[13] The moon's nature was not a settled matter. In many ways, it does not exhibit the uniformity ascribed to the other celestial bodies. For instance, even without a telescope, irregular light and dark patches – the "Man in the Moon" – are visible on the lunar surface, and were attributed to accidental variation in the luminescence of the aether composing the lunar body. The point, though, is that such questions were the concern of natural philosophy, not mathematics. See Miller (2018).

[14] See Distelzweig and Ragland in this volume (ch. 11).

conjoined to those effects. A *negotiatio intellectus* then elucidates the nature and operation of the cause, such that a final *demonstratio propter quid* can deduce the original effects from the newly understood cause. The legitimacy and details of this method were still in dispute in the early seventeenth century. One plausible interpretation of the method replicated the medical use of empirical "trial" (*peira*) to test causal hypotheses as a means of settling on the true cause from the available alternatives in the *negotiatio*.[15] However understood, the *regressus* inserted empirical reasoning into natural philosophy insofar as it relies on observations. Notably, Galileo himself studied the *regressus* method, probably when he was preparing his first university lectures.[16]

The philosophers' move toward mathematical method was mirrored by developments among mathematicians. Their social status had risen, largely due to the increasing importance of civil and military engineering and the novel technicality of the decorative arts, especially in perspective, while the humanistic recovery of classical texts, such as works by Archimedes and Pappus and the pseudo-Aristotelian *Mechanica*, enhanced scholarly interest.[17] In princely courts, figures like Frederico Commandino, Girolamo Cardano, Niccolò Tartaglia, and Guidobaldo del Monte made names for themselves as mathematical practitioners, formalizing methods, and producing treatises and translations. These humanistic mathematicians mostly adhered to the model of subordination, deriving mathematical results from unexamined physical principles. Even when they debated physical subjects, such as the causal basis of the law of the lever, they did so without engaging philosophers, or in direct opposition to them.[18] In the universities, meanwhile, professors of medical astrology gradually became responsible for a wider breadth of mathematical subjects.[19] Galileo, for instance, owed his post at Padua to the need for instruction in mathematical engineering, though he was also responsible for teaching astronomy. In this context, the approach to other disciplines was more conciliatory. For example, the mathematics faculty at the Collegio Romano, led by the astronomer Christoph Clavius, lobbied to incorporate the mathematical sciences in the *ratio studiorum* of the

[15] See Ragland (2017); Miller (2018); Sgarbi (forthcoming).
[16] Wallace (1992a); Wallace (1992b). [17] Peterson (2011).
[18] For example, Benedetti's *Demonstratio proportionum motuum localium contra Aristotelem et omnes philosophos* (Demonstration of the proportion of local motion against Aristotle and all philosophers) (Benedetti 1554; Drake and Drabkin 1969, 145). See also Van Dyck (2006).
[19] Carugo (1984); Laird (1986); Biagioli (1989).

Jesuit schools, thereby integrating them with the natural philosophical training that preceded higher studies.[20]

As for astronomy in particular, the advent of Copernicanism motivated practitioners to lean toward natural philosophical speculation. A moving earth posed questions about the nature and structure of the heavens, and astronomers debated the empirical and physical merits of the Ptolemaic, Copernican, and, later, Tychonic systems. Extensive observations of celestial phenomena, particularly the nova of 1572 and comet of 1577, seemed to show that qualitative change did occur outside the lunar sphere and that the heavens could not consist of solid aetherial orbs. In response, for instance, Tycho Brahe was led to posit heavens filled with fluid aether. Johannes Kepler went further, developing a "physical astronomy" that reconciled his proposed elliptical orbits with both Brahe's voluminous observation records and a new causal mechanism, based on the magnetic natural philosophy of William Gilbert.[21]

These developments contributed to the sophistication and autonomy of the mathematical sciences, and simultaneously brought them into contact and competition with natural philosophy, especially in the universities, which in turn occasioned reactionary defenses of the existing order. In *De certitudine mathematicarum disciplinarum* (1565), Alessandro Piccolomini defended the traditional preeminence of natural philosophy by amplifying the Aristotelian rejection of *metabasis*. He argued that mathematics fails to capture the causality of the natural world, and since the proofs of a genuine science must express causes, mathematics cannot produce *scientia* at all. Therefore, Piccolomini concluded, quantitative reasoning is of no use in the study of nature – mixed mathematics is of merely practical value.[22]

Piccolomini's thesis occasioned a lively debate, with tracts on every side of the issue, which continued through Galileo's time, and he was surely aware of it. What is significant here is that the debate brought into question the relationship of mathematics to natural causes, and this questioning broached the possibility that the natural world *is* governed by mathematical necessity.

[20] Dear (1987); Lattis (1994); Dear (1995); Crombie and Carugo (1996); Baldini (2003). To this one might also add the cosmological speculations of people like Cusa and Bruno, who were working in a humanist and Platonic context predominantly outside the bounds of both Scholastic natural philosophy and mathematics, though these are of less interest here, since their work was not predominantly empirical in the way that astronomy was.

[21] Stephenson (1994); Miller (2008).

[22] Piccolomini (1565). See Giacobbe (1972). For discussion of the entire controversy, see De Pace (1993).

Arguing against Piccolomini, Galileo's predecessor at Padua, Pietro Catena, defended the epistemic power of mathematics by contending that the logical necessity instantiated in mathematical proof determines the causal structure inhering in physical objects.[23] For Catena, then, the usual order of subordination is reversed. Physical sciences derive their certainty from the logic of quantities and magnitudes *per se*. Mathematics is "not only most certain in its own genera, but will shine its light and certainty on all the other parts of philosophy whatsoever."[24] In a mixed science, then, it is the mathematics, not the physics, that "render[s] manifest and with exactness all the properties of the objects of such sciences."[25] Again, the old disciplinary boundary blurred. Quantitative reasoning became informative in the study of nature.

Empiricism

Some modern scholars have viewed Galileo's approach toward Aristotelian natural philosophy as antagonistic, in keeping with the attitude of the court mathematicians of the previous generation, some of whom were his teachers and sponsors.[26] But Galileo held a university post, and seems to have been genuinely trying to engage his philosopher colleagues, even if he disagreed with them. While *Sidereus nuncius* was clearly a work of astronomy – a "mathematical" science alongside mechanics, harmonics, optics, and so on – written by the "public mathematician of the University of Padua," the title page also declares a "starry message," "presenting to the gaze of everyone, but especially Philosophers and Astronomers, those things observed by Galileo Galilei."[27] Its opening paragraphs address "all seekers of true philosophy,"[28] and in the text itself are several passages noting subjects of "philosophical" interest, and correcting "philosophical" opinions about the nature and arrangement of Earth and the celestial bodies.[29] Galileo thus envisions himself in discourse with philosophers, as well as astronomers, following the pattern set by previous mathematicians and astronomers.[30] Indeed, Galileo successfully used the book to negotiate a post as mathematician *and* philosopher to the Tuscan Grand Duke, exchanging his aspiration

[23] Giacobbe (1973); Giacobbe (1981). [24] Catena (1563, 2^{v}). [25] Catena (1563, 7^{v}).
[26] Most importantly, Drake (1976b).
[27] Galilei (1610, title page); Galilei (2004, title page).
[28] Galilei (1610, 7^{r}). Compare Tycho Brahe's *De nova stella* (1573), advertised as a "mathematical contemplation" (*contemplatio mathematica*) (Brahe 1573).
[29] See, e.g., Galilei (1610, 7^{r}, 5^{v}, 14^{v}).
[30] Galileo himself had previously entered into debate with philosophers regarding the supernova of 1604.

for the real thing, and demonstrating in his person the erosion of the boundaries between the disciplines.[31]

Features of the *Sidereus nuncius* exemplify the broader disciplinary developments that permitted such interdisciplinary engagement. Even as he "corrects" philosophical opinions, Galileo adheres to the methodological expectations of a philosophical audience. This is most evident in his discussion of earthshine – the "pale light of considerable brightness" that sometimes suffuses the part of the Moon's face not directly illuminated by the Sun. (This was not a telescopic discovery, but Galileo included his treatment since it was closely related to the book's other subjects.) The source of this light cannot be directly observed, so in order to "assign the cause," Galileo must present a causal demonstration, entering the territory of natural philosophy. That presentation conforms to the *regressus* structure.

First, he details the effect, listing numerous observations. He then lists light sources of sufficient brightness that are always found in association with the effect, and which other authors had proposed as the cause of the effect. This apparent *demonstratio quia* establishes that one of these is the cause, though the precise causal power at work is not yet known. In what amounts to a *negotiatio intellectus*, Galileo proceeds to eliminate the possibilities included in the *quia*. Significantly, he does this based on observations. For instance, the light cannot be coming from the Moon itself or the stars,

> For if this kind of light were the Moon's own, or were contributed by the stars, the Moon would retain it, particularly in [lunar] eclipses, and would show it then, when left in an unusually dark sky, but this is contrary to experience [*adversatur experientiae*].[32]

Similarly, the secondary light cannot be sunlight filtered through the Moon, for then it would be seen during solar eclipses, and it is not. Rejecting alternatives leaves the Earth as the only possible source of the light. So the secondary light is *earthshine*, sunlight reflected off the Earth's surface, then reflected off the Moon back to terrestrial observers. This understanding of the mechanism of the cause then allows Galileo to "explain the matter more clearly,"[33] and deduce, in a *demonstratio propter quid*, additional aspects of the observed effect; in particular, why the effect is strongest between the new

[31] Galileo's negotiation with the Tuscan court was an earnest, rather than cynical, attempt to obtain better remuneration and prestige – he was really trying to be a philosopher. This comports with his assertion, as an old man, that he was "always a Peripatetic in method" (Galilei 1890–1909, 18:248; Wallace 1992a, 295).

[32] Galilei (1610, 14^{v}); Galilei (2004, 14^{v}). [33] Galilei (1610, 15^{r}); Galilei (2004, 15^{r}).

and quarter moons.[34] From this, Galileo concludes that the Earth is not "the place where the dull refuse of the universe has settled down,"[35] but luminous like a celestial body.

Here, Galileo combines the methods of astronomy and natural philosophy by inserting the empirical hypothesis testing typical of the former into the *regressus* formula used by the latter. He thus adopts the argumentative trope of natural philosophers even while using observations to evaluate *causal* (not merely mathematical) hypotheses about the nature of the secondary brightness, and therefore the Earth. In so doing, Galileo moves across the traditional disciplinary boundaries at precisely the point of weakness – the *regressus* – created by his recent predecessors.

Some respondents sought to re-assert the disciplinary bounds Galileo transgressed, undermining the natural philosophical significance of his observations. For instance, the Aristotelian philosophy professor at La Sapienza in Rome, Giulio Cesare La Galla, proposed in his *Phoenomena in orbe Lunae* "to have a physical, not mathematical, disputation,"[36] in contradistinction to *Sidereus nuncius*. La Galla extensively reports and affirms Galileo's telescopic observations – he even allows that the "telescope shows" that the Moon seems to have "mountains and valleys"[37] – and he interprets Galileo's attempt to "assign the causes" of the observations as an instance of *regressus*. However, La Galla holds that Galileo's *propter quid* is improper, since his argument is "mathematical," and thus cannot generate causal premises:

> Mathematics does not, in considering the celestial motions and phenomena, proceed by *demonstrationes propter quid* – that is, from cause to effect – but by *demonstrationes quia*, from effect to cause.[38]

That is, the mathematical sciences, including astronomy, cannot demonstrate causes on the basis of observed effects, as Galileo claims. La Galla's argument is that sensation is often deceptive, and must be "corrected" by the "right reason" provided by natural philosophy.[39] "Therefore, to avoid such deceptions," La Galla concludes, "I believe offering a physical disputation is more prudent than a mathematical or optical one."[40] And since natural philosophy shows that the celestial nature of the lunar body is incapable of unevenness, the Moon cannot be mountainous, however much it appears so.

[34] I.e., because this is when the Moon faces the illuminated part of the Earth.
[35] Galilei (1610, 16[r]); Galilei (2004, 16[r]).
[36] Galilei (1890–1909, 3-1:323). Original is La Galla (1612).
[37] Galilei (1890–1909, 3-1:377).
[38] Galilei (1890–1909, 3-1:338–339).
[39] Galilei (1890–1909, 3-1:324–325).
[40] Galilei (1890–1909, 3-1:325).

Once again, physical reasoning provides the universals that should "correct" the particular observations, but observational astronomy cannot correct natural philosophy.

Other readers of *Sidereus nuncius* celebrated Galileo's disciplinary transgression. In his *Dissertatio cum Nuncio sidereo* (1610), Kepler embraced Galileo's conflation of the sciences. For Kepler, the book was at once astronomical and philosophical, and its observational reports were obviously useful to establishing and refuting claims in natural philosophy – in accord with the project of "physical astronomy" he had advocated in the *Astronomia nova* (1609). Kepler heard himself invited by the passages where Galileo addresses the disciplines together, and quoted them.[41] Kepler also aimed to elaborate "all the parts of philosophy which are threatened with destruction or clarified by your [i.e., Galileo's] 'Messenger'," [42] taking the astronomical observations as natural-philosophically decisive. Likewise, the mathematics faculty at the Collegio Romano saw the *Sidereus nuncius* as allied with their efforts to raise the import of the mathematical sciences. They therefore heartily supported Galileo's work, honoring him with a banquet in 1611, even if they did not entirely adopt his Copernican conclusions.

In the manuscript *Contro il moto della terra*, Galileo's frequent antagonist Ludovico Delle Colombe also remarks upon the disciplinary distinctions at issue, noting that the debate over Copernicanism is about physics, not mathematics. Yet unlike La Galla, Delle Colombe accepts Galileo's use of observations as grounds to reject physical principles, and emulates it – in order to reach an opposite conclusion: the Earth is not capable of the motions Copernicanism ascribes to it. This "philosophical" argument proceeds *empirically*, on the basis of discrete experiences. "How ever, by their faith," Delle Colombe asks, "will the Copernicans, who hold the Earth moves, respond to this argument founded in experience [*fondato nell' esperienza*]?" [43] Whereupon Delle Colombe points out, for instance, that cannonballs shot east and west are seen to travel equal distances, that birds can fly equally easily in any direction, that people and buildings are not thrown away from the supposedly rapidly spinning earth, and that dropped bodies fall straight down.[44] So even if he disagrees with Galileo's conclusions, Delle Colombe accepts that claims about the nature of the Earth can be refuted by empirical tests, not just rational argument (though Delle Colombe thinks that works in his favor, too). Significantly, Galileo later felt compelled to respond to Delle

[41] Kepler (1965, 11). [42] Kepler (1965, 14). [43] Galilei (1890–1909, 3-1:255).
[44] Galilei (1890–1909, 3-1:255–265).

Colombe's experiences in the *Dialogue concerning the Two Chief World Systems*, showing that they are not decisive.[45]

In the aftermath of *Sidereus nuncius*, Galileo's audience explicitly recognized, both positively and negatively, his rational-empirical method, constructed by interpenetrating astronomy's empirical hypothesis testing and natural philosophy's causal investigation. Even some of his opponents came to view the use of empirical tests, including contrived experiments, as one (but not the only) means of confirming or rejecting explanatory principles. This commitment to empiricism featured prominently in Galileo's later work, and subsequently in that of his disciples; for instance, in the Accademia del Cimento, whose mission was the experimental testing of physical hypotheses.[46] Eventually, the Galilean method would become intertwined with natural history and the independently conceived Baconian method, which added initial inductive steps by which hypotheses are generated from experience before being tested against observations. This became the primary root of modern science's commitment to the evidentiary value of observation and experiment.

Mathematization

A similar breaking of disciplinary boundaries can be seen in Galileo's application of geometry to nature in the *Sidereus nuncius*, where Galileo gives quantitative descriptions of his instrument and observations and offers one extended geometrical proof. The proof is simple – it is merely an extension of an optical method for determining the dimensions of distant objects using known distances and angles between sightings, which was commonly taught by practical mathematicians, including Galileo.[47] Assuming that the light from the Sun travels in straight lines and using some basic trigonometry, Galileo shows that the position of bright spots on the Moon – which he assumes are the tops of lunar mountains – relative to the terminator dividing the lit and shaded portions of the lunar face entails that the height of the tallest mountains is at least four Italian miles. "We are therefore to conclude," writes Galileo, "that it is clear that the prominences of the Moon are loftier than those of the Earth."[48]

While the technical details of the proof itself were unassailable, Galileo's conclusion was contested. At issue was the applicability to the natural world of geometrical reasoning about quantities. Recall that, according to the

[45] Galilei (1967, 126–133). [46] See Middleton (1971); Boschiero (2007).
[47] See Galilei (1890–1909, 2:414–428); Galilei (1977, 30–37). [48] Galilei (2004, 14[r]).

traditional subordination of mathematics, mathematical reasoning did not produce natural science without the addition of physical premises imported from natural philosophy, since physical necessity is autonomous from and superior to mathematical necessity. On the traditional view, then, Galileo is guilty of a *petitio principii*. *Given* the existence of lunar mountains, the mathematical derivation measures their height. But the existence of mountains is a question decided in natural philosophy, not mathematics. So Galileo's proof in itself decides nothing at all about mountains on the Moon, let alone their height.[49]

Again, some respondents defended the traditional view regarding the supremacy of physical reasoning. Delle Colombe wrote:

> And who does not know that it is necessary to be more the philosopher than the mathematician, and to know the first science [*scienza*] more than the second, in order to be able to judge correctly whether these hypotheses [*teoriche*] and mathematical demonstrations [*matematiche demostrazioni*] can be applied properly to matter, to place, and to motion; since judgment of all three appertains to natural philosophy, and not to mathematics, which abstracts from these natural qualities?[50]

That is, "mathematical demonstrations" are not probative in natural philosophy, since it is the latter that decides the applicability of the former. The philosopher La Galla, meanwhile, challenged the notion that mathematics could be regulative in natural philosophy by insisting on the bar against *metabasis*. He argued that mathematical considerations are more "remote from our senses and the only source of our knowing."[51] For both authors, mathematics only reflects the necessity of abstract quantities, distinct from the "natural qualities" of the objects of our senses. They both dismissed Galileo's proof on the grounds that the Moon must be a perfect sphere – and then offered alternative explanations of the observed spots on that assumption.

But Galileo was operating in a context informed by the inversion of the ordering of the sciences that the debate *de certitudine mathematicarum* had produced. Recall Catena's position, on which natural causality is beholden to the same logical necessity that yields mathematical proof. Seen in this light, a chain of mathematical inferences *can* recapitulate a chain of physical causes. Thus, once the observations are reduced to quantitative measures according

[49] This was a main criticism in Anonymous (1890). For more on Galileo's argument, see Gingerich (1975); Hartner (1975); Ariew (1984); Shea (2000); Ariew (2001); Wilson (2001); Spranzi (2004).

[50] Galilei (1890–1909, 3-1:255). [51] Galilei (1890–1909, 3-1:323).

to the principles of optics, the geometrical manipulations of those quantities demonstrate what produces those observations – geometry dictates what is physically happening on the Moon. So, for Galileo, the proof shows that the observed bright spots are really caused by peaks four miles tall. The mathematics permits him to "speak about their size and to show the ruggednesses of the Earth's surface are far smaller than those of the Moon's,"[52] even though the unevenness of the lunar surface is never directly seen.

The methodological effect of this blending of natural philosophy and mathematics is also visible in the contretemps that followed the publication of *Sidereus nuncius*, where echoes of the debate *de certitudine* resound among Galileo's supporters. Asserting the power of mathematics over physics in response to yet another critical treatise,[53] John Wedderburn wrote,

> It certainly seems to me the same thing to proceed mathematically and to imitate those disciplines, in which nothing is not defined, everything coheres, and which were the only sciences, in ancient times and according to Aristotle, to deserve the name of method, from which all other sciences, if they have any perfection, borrowed their method, and by proportions, bind in their orderings the principles and results to the appearances of their object. Thus it is that Aristotle's whole philosophy swarms with geometrical examples, wherever something difficult and by any other way inexplicable offered itself. But the philosopher is also seen to have extracted from these disciplines that part which belongs to the principle object of the expounders of the sciences ([and] of which discovery he boasts more than of the others), to such an extent that logic, insofar as it is an instrument of knowing, ought to be called the legitimate offspring of mathematics.[54]

This is to say that Aristotle's canons of scientific method are "extracted" from the autonomous practice of mathematics. Like Catena before him, Wedderburn is here reversing the traditional ordering of mathematics and natural philosophy – the physical sciences derive certainty from the logic of mathematics. The necessity that pertains to mathematics is the *same* necessity that pertains to natural philosophy. This implies that mathematics and nature itself are law-like *in the same way*, and physical phenomena are subject to mathematical laws.

For his own part, Galileo agreed. To the La Galla passage about the "remoteness" of mathematics from physics, Galileo added a postil:

> It is asked whether the mathematical considerations of the heavens are physical or mathematical. If in fact they are [only] mathematical, the times

[52] Galilei (1610, 13^{r}); Galilei (2004, 13^{r}). [53] Horky (1610). [54] Galilei (1890–1909, 3-1:169).

> of eclipses, conjunctions, etc., will not correspond with sensible and real conjunctions. It is no less ridiculous to say that geometry is not reflected [*non respondere*] in material things, or if one were to say that arithmetical results are not reflected in sensible bodies, and, e.g., the rules of teaching and coordinating exercises are not reflected, when we consider bodies of soldiers.[55]

In other words, *of course* nature answers to mathematics. The heavens obey astronomical theories in the same way that sensible objects can be counted and soldiers answer to their orders. It would be "ridiculous" to assume otherwise.[56]

Galileo's mathematical reasoning in the *Sidereus nuncius* reached into the realm of natural philosophical conclusions through the methodological openings made by the previous generation, especially in the debate *de certitudine*, where the Aristotelian subordination of mixed mathematics was questioned and even inverted. Similar transgressions of Aristotelian bounds are also apparent in other parts of Galileo's corpus. They are prevalent in his earlier work on mechanics, later refined and published in the *Discorsi* (1638),[57] wherein he sought mathematical expressions of, *inter alia*, the law of free fall and the trajectory of projectile motion. Moreover, the methodological innovation recognized in the criticisms and the defenses of *Sidereus nuncius* had a profound effect on natural philosophical discourse. Galileo's guiding assumption that mathematics expresses natural necessity paved the way to the conception of physical bodies as quantities central to the mechanical philosophy, and to the eventual expectation that mathematics *should* be used to express natural laws. One can therefore draw a line from the debate *de certitudine* through the *Sidereus nuncius* to modern science's mathematization of the world.

Conclusion

The above narrative is meant to illustrate the disciplinary admixture that gave rise to modern science. In one sense, there was nothing particularly special about *Sidereus nuncius*. It continued trends that had already permeated the intellectual contexts in which Galileo operated. On the other hand,

[55] Galilei (1890–1909, 3-1:323–324).

[56] Of course, this is also the thrust of his more famous assertion in the *Assayer* (1623) that the "book of nature" is "written in the language of mathematics" (Galilei 1890–1909, 6:232; Galilei, et al. 1960, 183–184). See also Biener (2004).

[57] Galilei (1638).

Galileo's fortuitous and astounding telescopic discoveries made the book the center of a lively discourse, and this made the new, mixed framework "stick." The debates that arose about the nature and motions of celestial and terrestrial objects as a result of *Sidereus nuncius* were shaped by its implicit expectations, which combined methodological aspects of the mathematical and natural philosophical disciplines. Participants accepted observations as evidence and the decisiveness of mathematical proofs in physics. Those that did not were eventually left behind, outside the bounds of the new scientific tradition.

This does not imply that Galileo was fully cognizant of his methodological move. He was just rushing to get his book published, not trying to contribute to the debates *de certitudine* or about the *regressus* method. But he was writing in a context informed by these controversies, and they had fractured the social, practical, and methodological boundaries separating the mathematical disciplines from natural philosophy proper. *Sidereus nuncius* catalyzed the formation of the new science out of the resulting mixture.[58]

[58] Thanks to Marius Stan for very helpful comments.

18

Instruments and the Senses

PHILIPPE HAMOU

Telescopes and Microscopes as Philosophical Instruments

Optical instruments – telescopes and microscopes – appeared in Europe in the first decades of the seventeenth century. First discovered in a more or less empirical way, then improved through a mix of bricolage and optical theory, they disclosed newly visible worlds: one beyond the dome of the sky populated with new stars and planets, another crawling with unexpected forms of life among the least particles of matter. The history of these remarkable instruments can be addressed in many ways, all rich and instructive. Understanding their invention and subsequent development calls for inquiries into the material and technical conditions that made them possible and into the changes in optical theory that were needed to account for them and improve their performance.[1] In parallel, an assessment of their scientific importance requires detailed histories showing how actual telescopic and microscopic discoveries were integrated into the body of astronomical knowledge, on the one hand, and into theories of matter, natural history, physiology, and anatomy of plants and animals, on the other.[2]

[1] On lens-grinding techniques and technical constraints on the productions of the first instruments, see especially Daumas (1953); Willach (2001); Burnett (2005); Willach (2010). For a "European" perspective on the history of the telescope, see Bucciantini, et al. (2015). For a recent and up-to-date general history of optics, see Darrigol (2012). For classical historical accounts, with important chapters on the theory of the telescope and microscope, see Smith (1738); Priestley (1793). Early microscopy is dealt with in Bradbury and Turner (1967). Optics and telescopic theory in Kepler are addressed specifically in Straker (1971); Malet (2003); later seventeenth-century developments, in Malet (2010). For Huygens's dioptrics, see Hashimoto (1989). Barrow's and Newton's contributions to the theory of optical imagery are explained in Shapiro (1990).

[2] Studies of the astronomical impact of the telescope include King (1955); Van Helden (1985); Winkler and Van Helden (1992); Hoskin (1999). On early microscopy's relation to atomism and theory of matter, see Lüthy (1995). Classical studies of its impact on life sciences include Fournier (1996); Ruestow (1996).

In this chapter, however, the focus will be primarily on the philosophical stakes that, from the start, surrounded the invention and use of the instruments.[3] In the seventeenth century, telescopes and microscopes were indeed eminently *philosophical* instruments. They were so, of course, in the early modern sense of the term 'philosophical', as they contributed to "natural philosophy," the science of nature. But, and this is the sense that we shall consider here, they were also philosophical in that they had epistemological, anthropological, and, to some extent, metaphysical lessons to offer. Optical instruments affected the modern understanding of what people are up to when they do science – i.e., look at the book of nature and try to understand it. They prompted scholars to reflect on the nature and *scale* of the objects they were investigating, on the epistemic status of sensory experience, and on the hopes that could be entertained for the future of scientific enquiry. They also affected humanity's self-representation of their own senses, and, insofar as human knowledge is indebted to sensory, and especially visual, experience, of humans' capabilities as knowing beings.

In considering these philosophical stakes from a broad perspective, one is struck by a marked polarity. On the one hand, the telescope and the microscope were epoch-making symbols of a conquering modernity. They showed that men had the power and ingenuity to transcend their human sensorial condition; they could see beyond the range of their natural senses and glance into a world that had remained hidden to all previous generations of enquirers. As René Descartes wrote in the opening lines of the *Dioptrics*, "those marvelous telescopes ... carrying our sight much farther than the imagination of our fathers was used to going, they seem to have opened the way for us to attain a knowledge of nature much greater and more perfect than our fathers had."[4] Celebrating the virtues of the instruments, the moderns often used the metaphor of the great discoverers of the previous century. Galileo Galilei, armed with his telescope, was compared to Christopher Columbus or Amerigo Vespucci, disclosing new worlds in the heavens.[5] A few decades later, the power of the microscope was compared with that of the telescope. Both offer a perspective on an open-ended progress, a rich harvest of new discoveries. As Nehemiah Grew, a pioneer

[3] In this line of enquiry, decisive inputs are provided by Wilson (1988) and its development in Wilson (1995). For an anthropological approach to scientific instruments, see in particular Kutschmann (1986). The epistemological stakes are the main focus of Hamou (1999) and Hamou (2001). The present chapter partly draws on these studies.

[4] Descartes (2001, 65).

[5] As, for example, in Johannes Faber's Latin poem, written in praise of Galileo (Galilei 1890–1909, 6:205). Further references are in Nicolson (1956); Kepler (1993, 105, n. 180).

of early microscopy, exclaimed on the opening page of his *Anatomy of Plants*: "we are come ashore into a new World, whereof we see no end."[6]

But on the other hand, and in stark contrast with this optimism and progressivism, optical instruments taught a lesson in relativity that went well beyond the usual skeptical doubts about the senses. Showing that the human eye, and probably also the human understanding, are not adequate to the immensity, subtlety, and complexity of natural productions, they made the intrinsic limitations of our sensorial, and perhaps mental, equipment manifest. Even when assisted by artificial aids, our bodies and minds are overwhelmed on all sides by unfathomable dimensions of existence. This was the new world created by the instruments, a world not anymore measured by our senses, and even less proportioned to them. As John Locke wrote in his *Essay concerning Human Understanding*,

> the intellectual and sensible world are in this perfectly alike: that part which we see of either of them holds no proportion with what we see not; and whatsoever we can reach with our eyes or our thoughts of either of them is but a point, almost nothing in comparison of the rest.[7]

In order to give a proper account of the philosophical impact of the instruments in early modern times, it is important to keep in mind both sides of the story, the optimistic and the pessimistic. A possible way of connecting these two apparently disjointed parts, is to consider some aspects of the long-debated history of the "first invention" of the telescope. The complex pattern of empirical discovery and *post hoc* rationalization discernible here may indeed help us understand the reason why the philosophical legacy of the instruments was so ambiguous.

Empirical Invention and Rational Reinvention

On the opening page of his 1637 *Dioptrics*, Descartes told this interesting story:

> But to the shame of our sciences, this invention, so useful and so admirable, was found in the first place only through experiment and good fortune. It was about thirty years ago that a man named Jacques Métius of the city of Alcmar in Holland – a man who had never studied, although he had a father and a brother who made a profession of mathematics, but who particularly enjoyed making mirrors and burning glasses, even making some in winter with ice, as experimentation has shown we can do – having on that occasion many lenses of different shapes, happened by chance to look through two of

[6] Grew (1682, epistle dedicatory). [7] Locke (1975, IV.iii.23).

> them, one being slightly thicker in the middle than in the edges, and the other, on the contrary, much thicker at the edges than in the middle. And happily he hit upon the idea of placing them in the two ends of a tube, so that the first of these telescopes of which we are speaking was thereby made.[8]

Descartes's story may not be perfectly accurate,[9] but it does not seem to be too far off the mark. In any case, it offers guidance for discussing some supposed features of the first invention of the telescope. Recent research[10] has shown that both technical advances in craftsmanship (notably in spectacle-making, grinding technique, and the industry of glass production) and practical mathematical knowledge about the general effects of lenses and mirrors (of the kind one could find in books of "inventions and devices," such as those by William Bourne and Thomas Digges)[11] had their part in making the invention technically possible at about the turn of the century.[12]

Treatises of natural magic, such as Giambattista Della Porta's *Magia naturalis*, were also instrumental in bringing the idea of a telescope to life. They suggested various devices for enlarging letters or projecting images through mirrors and lenses; but even more importantly, they fostered a new conception of the relationship between man and nature: through human activity, nature could be forced out of her accustomed track, and exhibit unprecedented, unheard-of phenomena.[13] Descartes quite clearly hinted at these various developments when he signaled Metius's know-how, his relations to mathematicians, and his interest in quasi-magical devices such as burning mirrors made out of ice.

[8] Descartes (2001, 65).

[9] The story of the invention was told early on by Pierre Borel (1655), who established, rather than Metius, Zacharias Janssen, a craftsman (but also a counterfeiter), as the true inventor. However, a patent for the instrument was sought from the States General in The Hague by another craftsman from Middleburg named Hans Lipperhey (or Lippershey), and again soon after by Metius. Thorough research undertaken by De Waard (1906), and Van Helden (1977) has brought a rich harvest of new information, but no certitude on the name of the "true" inventor. It is possible that the Dutch artisans knew of an Italian prototype, constructed in the late sixteenth century. For an interesting account of how national pride, dubious evidence, and historical myths interplay in this complex historiography, see Zuidervaart (2010).

[10] See note 1.

[11] Digges and Digges (1571); Bourne (1578).

[12] The work of sixteenth-century mathematical practitioners has recently been the focus of attempts to revise the standard story of the "Dutch" invention. Some devices, such as the so-called "Elizabethan telescope" using a concave mirror, described in a letter by Bourne, have been considered as proof that the telescope was invented by Englishmen, long before the Dutch patents. Analogous claims have been made for an alleged sixteenth-century Spanish invention. See respectively Dupré (2010) and Settle (2011), whose careful considerations of the available evidence show that there is not much support for such claims.

[13] Eamon (1994).

To Descartes, the haphazard, empirical discovery of the telescope was nevertheless a scandal of sorts. Descartes considered his own contribution in the *Dioptrics* as a remediation of that scandal, a means to restore to science and method a right of institution and authorship over a modernity that was inappropriately born in a craftsman's workshop.

A similar idea had been suggested by Galileo and Johannes Kepler. Both claimed to have *reinvented* the telescope in a more "rational" way. Thus Galileo explained in the *Assayer*,

> we know that the Fleming who was first to invent the telescope was a simple maker of ordinary spectacles, who casually handling lenses of various forms happened to look through two at once . . . . But I, incited by the news mentioned above, discovered the same thing by means of reasoning.[14]

Galileo then explained how he "reinvented" the telescope, asking himself questions such as how many lenses these tubes should contain, or what sort of lenses would do the job of enlarging without confusing the object.[15] These rather rudimentary, purely qualitative considerations are clearly *post hoc* reconstructions. It could be the case that Galileo knew more about the theory of the telescope, but chose to hide his knowledge.[16] However, it is not likely that Galileo's optical knowledge in the first decade of the seventeenth century was much greater than what a mathematical practitioner of the sixteenth century could muster.[17] And in any case, it does not seem that Galileo in 1609 was aware of, or at least interested in, Kepler's advances in this domain.

Kepler, for his part, had better grounds for claiming a rational reinvention. In the weeks following Galileo's announcement of the telescopic discoveries, he wrote a masterly treatise on dioptrics in which he applied to the telescope the principles outlined seven years earlier in his groundbreaking optical treatise, the *Paralipomena to Witelo*. The 1611 *Dioptrics* was written in the form of a Euclidean mathematical treatise, advancing definitions, axioms,

[14] Galilei (1890–1909, 6:258–259).

[15] On Galileo's introduction of his telescope see Rosen (1951); Drake (1970b); Drake (1976a); Galilei (1992); Bucciantini, et al. (2015). For a thorough account of the intellectual context explaining Galileo's claims, see Reeves (2008).

[16] Zic (1999) claims that Galileo could not have his best lenses produced without prior knowledge of the law, later enunciated by Torricelli and Huygens, relating the (focal) length of the instrument to the radius of curvature of the objective. An exact understanding of refraction, however, may not have been necessary for arriving at an empirical approximation of this relation.

[17] For an assessment of the deep entrenchment of Galileo's knowledge and practice in sixteenth-century practical optics, see especially Dupré (2002).

and theorems. It thus proceeded in a synthetic way, the telescope being, so to speak, deduced from its geometrical causes.[18] We see Kepler taking sequentially the materials of a disassembled telescope and producing their theory: the piece of flat glass from which lenses are made and its refractive properties are investigated, then the convex and concave lenses and their effects *per se*. Afterward, the human eye is described as a natural *camera obscura*, and some psychological properties of vision are expounded. Kepler offers a geometrical explanation of the properties of these elements taken one-by-one (the convex objective lens, the convex or concave eyepiece, the eye), then, having nested them one over the other as in a tube, that of their composition. The choice and ordering of the parts strictly follow from what could be called the "manufacturing order" of the instrument – the telescope is gradually constituted and explained by stacking and adjusting the lenses before the eye. Here again it seems that the theorization, in spite of its geometrical sophistication, would not have been possible without the empirical exhibition of an *actual* instrument. A confirmation *a contrario* lies in the simple fact that the *Paralipomena* of 1604, where all the theory needed was ready at hand, did not spontaneously yield, without the need of an empirical solicitation, the invention of a telescope. Indeed, the *Paralipomena* had a "blueprint" of a Galilean telescope that is almost drawn in a single diagram, which Kepler uses to describe the opposite effects of convex and concave lenses (see Figure 18.1), without thinking of actually combining them.

Descartes was probably the most consistent in his proposal for a rational re-appropriation of the instrument. On his view, the fact that the telescope was still extremely imperfect betrayed its dubious origin in chance and experience. A more perfect instrument could have been invented if the question, "how could vision be perfected?", had been asked and dealt with properly, in a methodical way. The point of the 1637 *Dioptrics* was to illustrate the strength of a new method of invention, based on the model of mathematical invention – that is, problem-solving through the use of analysis.[19] Granting some basic features about the behavior of light and the anatomy of the eye, and some characteristics of vision (that is, "the givens," whose explanations are contained in the first six Discourses), the text then seeks to determine the different ways of perfecting vision, to examine their feasibility, and, if possible, to offer a pattern for the construction of the best of them (the

[18] Kepler presents the treatise as a "geometrical panegyric" of the instrument (Kepler 1611, 28).

[19] On Descartes's *Dioptrics* as a paradigmatic illustration of Descartes's "method," see Ribe (1997); Hamou (1999, ch. 8); Schuster (2000).

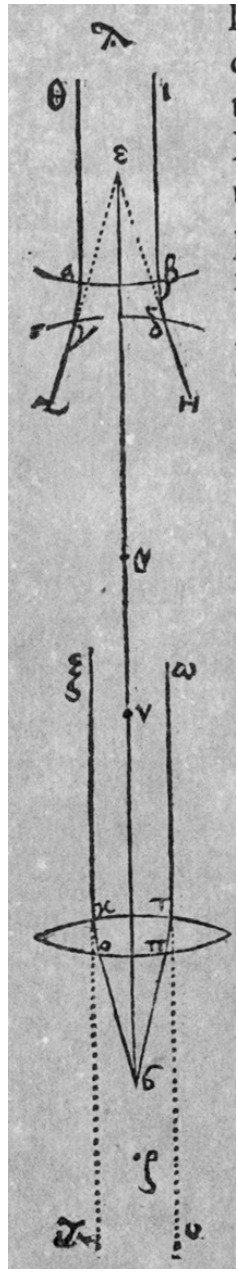

Figure 18.1. Kepler's diagrammatic "telescope"
Source: Kepler (1604, 202). From ETH-Bibliothek Zürich

"findings" presented in the last four Discourses). In this exposition, the actual artifact – the craftsman's telescope – no longer occupies the center of the analysis. It is no longer a model or an *explanandum*, as it still was in Kepler, but, pushed toward the margins of the text, it leaves room for an intelligible object, a blueprint or standard for an ideal telescope, which does not presuppose any concrete figure or shape, but which nevertheless perfects vision, allowing it to see farther and more distinctly.

In Discourse Seven, which envisions the different ways this perfect instrument could be made and discards the ones he judges impracticable, Descartes progressively comes to the idea of an extension of the aqueous body of the eye, which would increase the distance separating the back of the eye from the point of intersection of the rays coming from different parts of the object seen – this would *ipso facto* enlarge the retinal image. Descartes imagines three devices, of which only the last corresponds to a real telescope (see Figure 18.2). The first is a dovetail-shaped tube, filled with water and applied directly against the eye, with external surfaces shaped exactly like those of the eye. The second is constructed of solid glass, which does not have the same refractive index as the aqueous humor of the eye, and requires two refractions at the entrance and exit to have the same effect as the tube of water. The third is an empty tube with glass lenses at both ends, whose refractions produce the same effect as the front and rear surfaces of the previous device. The progression from one device to another is important because it corresponds to a gradual consideration of the technological conditions that allow the effective realization of the instrument. Each device is substitutable for the preceding one, performing exactly the same function, but the first is theoretically simpler, and thus makes more patent the true essence of the instrument, while the latter is, supposedly, the least inconvenient to make (the rest of the treatise will explain how to make it).

There are several quite remarkable features of this Cartesian "reinvention" of the telescope. One is the fact that it is supposed to be the example *par excellence* of a methodical enquiry. Although the *Dioptrics* is about an extraordinary instrument of empirical discovery, itself empirically discovered, the text's message is that true inventiveness owes nothing to experience and chance – it is purely a matter of rational (mathematical) analysis.[20] Another notable feature of the *Dioptrics* is its reordering of the subject matter of optics. "Dioptrics," the science of refraction through "diopters" or lenses, becomes

[20] Descartes explicitly denies that crafts could be a reliable source of invention in his *Regulae*, Rule 8 (Descartes 1964–1974, 10:397) .

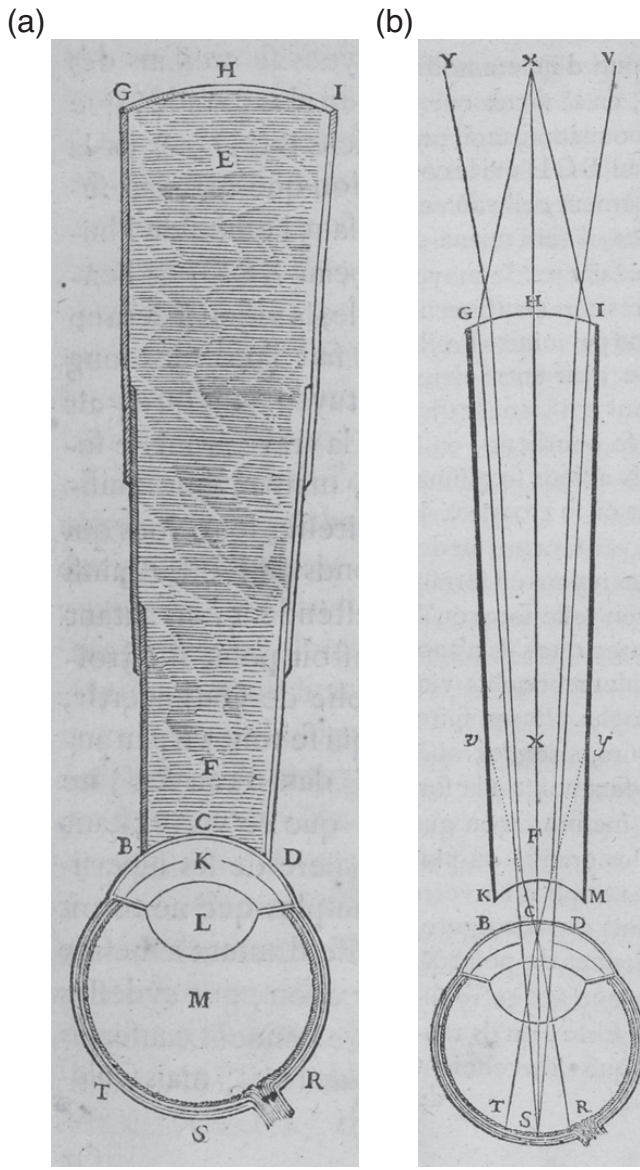

Figure 18.2. Descartes's first two devices for enlarging retinal images
Source: Descartes (1637a, 79, 82). From Bibliothèque Nationale de France

the privileged means for explaining the optical functioning of the eye. In medieval textbooks and optical treatises, such as Alhazen's and Witelo's,[21] the study of reflection and refraction always came *after* the study of direct vision, as it was supposed to explain how sound, direct vision could be misled by mirrors or lenses. But Kepler had shown that one cannot understand direct vision – that is, what is supposed to be true vision – without first coming to terms with refraction. The theory of the telescope makes this even clearer. The eye has a lens and a diaphragm (the lens and the iris), and, as Descartes's *Dioptrics* showed, there is no substantial difference in principle between a naked eye and an eye equipped with an instrument. So the optical body of the eye (its lens and aqueous humor) is like a natural telescope, and the ideally conceived artificial instrument is only a way of reconfiguring this natural telescope at will, so that it can apprehend things that it does not usually see. We must therefore think, as Malebranche suggests following Descartes's lead, that

> our own eyes are in fact only natural telescopes, that their humors have the same effect as the lenses; and that according to the situation they maintain between them, and according to the figure of the crystalline lens and its distance from the retina, we see the objects differently, so that one cannot assure that there are two men who see anything precisely the same size.[22]

Here we arrive at the crucial point from which paths diverge: The eye is an instrument, the instrument is an eye. This conceptual identification allows two quite different interpretations. On the one hand, the instrument fulfills the function of sense, but it does so in a better way. It is a sense, but as Galileo said, "a higher and nobler sense than the natural and common ones."[23] The instrument makes us see clearer, farther, and in more detail regions that were before too far away or too indistinct to give rise to any sort of certainty. It allows sense to accomplish its true function: making known its object directly, without any interpretation or hypothesis. But on the other hand, one could also say that telescopes and microscopes are *no more* than senses. The kind of information they provide is as relative, and as bodily dependent, as what we get from our natural senses. Far from revealing the true essences of things, senses only show their accidental properties. Here is why the meaning of the instrument is so ambivalent. It hinges on the epistemic value that one is ready to give to sensorial information. And reciprocally, this value is dependent on how the instrument is understood.

[21] Risner (1572). [22] Malebranche (1997, 28). [23] Galilei (1890–1909, 7:356).

Instruments as Perfect Senses

Let us consider the first idea: that the instrument is a perfect sense, or a perfected or *repaired* sense. As remarked above, this idea is clearly stated by Galileo. The success of his Copernican campaign depended on his ability to convince his audience that the telescopic experience was a well-constituted sensory experience, theoretically neutral, providing science a stable and undisputed empirical basis.

According to one traditional view, developed in the Aristotelian tradition and quite explicit in the optical textbooks of the time,[24] sensory experience, and especially visual experience, provides the understanding with true and certain knowledge of its object,[25] granted that the conditions of vision (such as the distance of the object seen, the illumination, and the physical state of the perceiver) are in a medium range between excess and defect. When these median conditions obtain, the act of vision can be repeated and confirmed, and, as a result, a judgment of sense[26] can be issued, assessing truly the properties or *intentiones* of the visible object. Vision errs only when it has to deal with *visibilia* under conditions outside the ordinary, for example when objects are too far away, too close, too obscure, or too luminous. It was usual to hold that the stars, on account of their extreme distance, were precisely the sort of objects whose properties could not be ascertained by sense, and about which one could only rely on reasoning and hypotheses.

Now, it seems quite clear that Galileo's strategy consisted not so much in stressing the novelty of the sensorial experience provided by the instrument, but rather its normality. His aim was to convince his readers (and among them philosophy professors, inculcated with the Aristotelian psychology of sense) that the telescope was a "perfect sense," precisely insofar as it has the virtue of re-establishing the right sort of conditions for the judgment of sense to take place. Contrary to naked-eye observation of celestial objects, telescopic phenomena were supposedly well defined, temporally coherent,

[24] Such as the *Opticorum libri sex* of the Jesuit Aguilonius. See especially the very Aristotelian account of how the first principles of science proceed from a well-constituted sensory experience (Aguilonius 1613, 215–216). On this, see also Dear (1991, 139ff.).

[25] One should note however that Scholastic authors were not of one mind on the issue of the relation between sense perception and certain knowledge. On the thirteenth-century debate about these issues, see especially Tachau (1988). Skepticism toward the senses was not uncommon among Scholastic authors. For recent accounts, see Lagerlund (2009); Adriaenssen (2017). Many thanks to Helen Hattab for pointing these references out to me.

[26] On this central notion of Scholastic and Renaissance psychology, see Summers (1987).

congruent with each other, stable, repeatable, and consensual. In other words, they were supposed to offer the same sort of internal, or phenomenological, confirmation as ordinary vision concerned with objects situated in the normal range of visibility, allowing sense to judge and ascertain the identity and true properties of the objects seen. In the observation of the mountains on the Moon, for example, what is seen at first – areas of light and shadow on the lunar face – does not yet attest to the reality of an uneven surface. One had to rely on the evolution of the aspects of the shadows to rule out competing interpretations of the dark spots perceived on the lunar surface. Thus, the telescope allowed an "ocular certainty" about objects (such as the surface of the Moon, or the Milky Way) whose nature had always been considered as inaccessible to the certification of sense perception.[27]

Not only does the "perfect sense" allow one to see better what was before perceived only confusedly, but it also allowed "autopsia" (from the Greek verb meaning "to see for oneself") of objects that had never been seen before, and that one could barely imagine existing. This was the case, for example, with Saturn's rings, which Robert Hooke, inspired by the Baconian "monodical" instances,[28] dubbed a "non-pareil of nature"[29] – a natural production that had no analogy with anything known. Of such "non-pareils," the microscope was also a great provider, especially after Antoni van Leeuwenhoek discovered a whole range of new little animals, swimming by the millions in a single drop of water.

Instruments thus contributed to the reactivation of the heuristic virtues of sensorial experience and observation. They forced natural philosophers to go back to reading the book of nature, where so many new, unexpected things were still to be seen and discovered. This ruled out a preconception, frequently held among Aristotelians, according to which the experiential content of science had been exhausted by the ancient naturalists (like Aristotle or Pliny), and was not anymore to be explored. In this, the instruments challenged ancient knowledge in a way that was very much reminiscent of the theoretical contribution of the voyages of discovery of the previous century, as presented in accounts like José de Acosta's *Natural and Moral History of the Western Indies*,[30] which stressed how uneducated sailors had been able to disprove ancient wisdom, such as the alleged uninhabitability of the torrid zone.

As is well known, such reactivation of natural history was also a Baconian project.[31] This explains why the telescope and the microscope found

[27] See the chapter by Miller in this volume (ch. 17). [28] Bacon (2000, I.45).
[29] Hooke (1726, 739). [30] Acosta (1590).
[31] See the chapter by Anstey and Jalobeanu in this volume (ch. 12).

a privileged place in the Baconian tradition of experimental philosophy. According to Catherine Wilson,

> the microscope was an instrument of enablement which permitted its devotees to maintain and extend a Baconian theory of the interpretation of nature. This theory was itself anti-occultist in its insistence of the replacement of the direct "reading" of nature by a dispassionate form of visual inspection coupled with processing by the Baconian inductive "machine."[32]

Bacon himself had been somewhat suspicious of the newly invented optical instruments. He counted them in his *Novum organum* among the "instances of first information," but deplored their limitations (lacking scales or measurement devices, the telescopes were not truly mathematical instruments) and in general remained reserved on the ability of the senses, aided or unaided, to give reliable information about nature.[33] In the second half of the seventeenth century, among a number of Bacon's heirs, these suspicions were swept away. The telescope and the microscope became "Baconian" instruments *par excellence*, and were praised as symbols of the new scientific age. Joseph Glanvill, a propagandist of the Royal Society, expressed a widely-shared view when he suggested that the instruments would one day restore mankind to its prelapsarian sensorial condition.[34] Adam before the fall, he explained in his *Vanity of Dogmatizing*, "needed no spectacles."[35] He could behold the stars as we do with the telescope and see magnetic *effluvia* as with a perfect microscope.

But it is certainly Robert Hooke, curator and chief experimenter of the Royal Society, who most vividly epitomized this new age of instruments. Not only was he a remarkable inventor, developing and promoting all kinds of new telescopes, microscopes, and other optical devices,[36] and the author of the amazing *Micrographia*, whose splendidly crafted illustrations give the dazzling feeling that one has entered a truly new world,[37] but he also gave the most explicit and consistent account of the new visual empiricism that the instruments helped promote. With Descartes, Hooke shared the idea that a true science of natural bodies ought to identify the internal mechanisms that govern the phenomena of the visible world. Hooke, however, did not share Descartes's confidence in the mind's ability to determine *a priori*, on the basis of hypothesis and analogical reasoning, what these insensible

[32] Wilson (1988, 89). [33] Bacon (2000, II.39).
[34] On original sin and science, see Harrison (2002). [35] Glanvill (1661, 5).
[36] On Hooke as mechanic and philosopher, see Bennett (1980).
[37] On the visual "rhetoric" of Hooke's illustrations, see Harwood (1989); Hamou (2001, ch. 4). Hooke's illustrations offer a good example of what Daston and Gallison (1999) characterize as the first stage in the history of objectivity: "truth-to-nature."

mechanisms were. Nothing could replace an actual inspection of the internal constitution of bodies, and Hooke thought the microscope, if brought to perfection, would be up to the task. In the *Micrographia*, he suggested that such mechanical explanations are already accessible in some cases. Thus, the sting of the nettle is nothing occult; it comes from the fact that the nettle leaf is covered with a network of small syringes filled with a corroding liquid. If cork floats, it is because it is composed of small cells filled with air. The fly does not adhere to the windows by the use of suction cups, but thanks to minuscule crampons it inserts into the microscopic pores of glass; and so on. These examples certainly did not go far into the subtlety of nature, but they gave a model of what should be sought in terms of physical explanation. The microscope allows us, as Hooke writes about the circulation of blood seen through the skin of an almost transparent larva, to "quietly peep in at the windows" and directly observe natural processes *in vivo*, without disrupting them, as we do "when we endeavor to pry into her secrets by breaking open the doors upon her, and dissecting and mangling creatures."[38] As Wilson suggested, commenting on this passage, microscopes allowed a form of detachment and receptivity to coexist with experimental engagement and the project of mastery of nature through knowledge and observation.[39]

We may call "instrumental autopsy"[40] the Hookean ideal of such direct visual access to internal mechanisms and constitutions. In his *Lectures of Light*, Hooke voiced his amazement at the almost incredible property of light radiation: "the least sensible Point of Body is able to affect the greatest *Expansum* of Nature."[41] The farthest or the smallest luminous points in the world do indeed affect our retina, even if we do not see them. Making these effects perceptible is just a matter of enhancing them up to the threshold of visual acuity, and this is precisely what ideally conceived microscopes or telescopes are supposed to do:

> it is evident that Light extends itself to the utmost imaginable Parts, and by the help of Telescopes we collect the Rays, and make them sensible to the Eye, which are emitted from some of the almost inconceivably remote objects . . . nor is it only the great body of the sun, or the vast body of the fixed stars that are thus able to disperse their Light through the vast expansum of the Universe: but the smallest Spark of a Lucid Body must do the very same thing, even the smallest globule, struck from a still by a flint, which is as small as the point of a pin. . . . Nor is this to be limited to a point big enough to be sensible to the naked eye for by the help of a microscope . . .

[38] Hooke (1665, 186). [39] Wilson (1995, 38).
[40] For discussion of this concept, see Hamou (2004). [41] Hooke (1705, 77).

> one is able to distinguish parts that shine 1000 [times] smaller than we can distinguish with the naked eye . . . . So that hereby we are ascertained by our sense, that the least sensible point of body is able to affect the greatest Expansum of nature.[42]

Hooke's epistemic optimism was immense. Perfecting the senses by promoting telescopes and microscopes was not only to promote the natural history of phenomena, it was to advance toward a true causal physical science, capable of disclosing the hidden springs and wheels of nature – a perfect science that would make us able to see what we could now only hypothetically guess. If that could be done, the instruments would elevate men well beyond their present condition. As Hooke wrote in his *Discourse of Earthquakes*,

> By them only we are truly made Gygantick, and our eye from the little Ball of less than an Inch in Diameter is grown to be of fifty, sixty, nay a hundred Foot and more in diameter, and may be made able to do some thousands of times more than what our bare Eye alone without the use of such helps can perform.[43]

Contingency of Sense Perception and "Corpuscular Pessimism"

I suggested above that the "perfect sense" interpretation of optical instruments was counterbalanced by another, less optimistic view of the instruments and their powers. The instrument was indeed a sense, but, precisely for that reason, its contribution to physical knowledge had the same sort of limitations as sensation in general. It showed appearances and superficies, but was incapable of offering reliable information about the intrinsic and constitutive properties of the objects seen. It may even be said that, once the sense organ was understood as something that was itself a kind of natural instrument, the old conception of sensory perception as an immediate and true acquaintance with its object was no longer sustainable. Particular spatial properties – magnitudes or figures – are highly dependent on how the instrument is designed, and the same object – for example, a planet in the sky – may present to the eye utterly different appearances, according to the type of "eye" or "telescope" applied to it. It will successively be a luminous and glittering point; a small, perfectly smooth, and uniform disc; or a rugged object with

[42] Hooke (1705, 77). [43] Hooke (1705, 358).

shaded spots or terrain. This was also true for the seemingly "true" colors of macroscopic objects that could transform or simply vanish when seen through the microscope.

Without doubt, optical instruments were not needed to understand that sensorial qualities are somewhat relative. Spatial properties change by the simple effect of changing perspective, and certain well-known pathologies, such as jaundice, affect our perception of colors. Nevertheless, the robust confidence in sense perception that prevailed in the medieval theory of vision was not really threatened by such doubts, because of the doctrine, already alluded to, according to which the sense of vision has immanent norms that allow it to correct itself, and judge correctly color and spatial properties. Size, for example, may be badly estimated in conditions of visibility that are outside the norm (this is the case, for example, when we attribute to the Sun, a body too far away for correct judgment by the senses, the approximate diameter of one or two feet), but under normal conditions we do not let ourselves be abused by perspective changes: we do not see familiar bodies change in size when we approach them, we see them simply maintaining their size while being closer to us. It thus seems that the judgment of sense was able to identify something like a "visual essence" of things, despite changes in aspect. It could ascertain the "true" shape, size, and configuration of the object seen. The same went for the colors. There are certainly pathological phenomena that could make us see them abnormally, but precisely because these phenomena are pathological, they do not concern normal vision, which does not err but informs us with certainty about its proper visible, the *true color* of objects.

However, through optical instruments in the early modern period, the old skeptical trope of perceptual relativity was strengthened greatly. It is not only the *conditions* of vision that were revealed to be contingent and mutable, but the *organs* themselves. For one thing, it became clear that the "sensorial norm" itself is contingently defined, and depends on the dispositions of the bodily organs. By no means is it an absolute norm attached to the objects and conditions of the experiential situation, and independent of these dispositions. To other dispositions, such as the artificial ones provided by the optical instruments, would correspond other standards. The mite probably does not see things of the same size as us, because the tiny objects which concern its immediate existence, and which we would not see without a microscope, must appear to it large and detailed enough to allow her to recognize, pursue, or flee them. Malebranche comments,

> But if we had eyes constructed like microscopes, or rather, if we were as small as mites, our judgements about the size of bodies would be quite different. For these tiny animals undoubtedly have eyes that can see both what surrounds them and their own body as though much larger, or as composed of a greater number of parts, than we see it, since otherwise they would not receive the impressions necessary for the preservation of their life, and thus the eyes they do have would be entirely useless to them.[44]

Locke and George Berkeley took up the Malebranchian theme of the microscope-eyes,[45] and both drew a similar conclusion: were human eyes configured like microscopes, all other things being equal, we would not be able to live in this world; it would present to us an aspect that has no relation to the demands of our body. Vision is a disposition that is supposed to answer to those needs. Its "normality" depends upon the appropriation of our organs to our *conservation*, but it may neither concern nor satisfy our desire for *knowledge*.

In the somewhat melancholic conclusion of his *Discourse concerning Telescope and Microscope*, written as a Gresham lecture in 1691, Hooke noted that the microscope did not stimulate vocations and enthusiasm anymore. He lamented that "tho' there has been some Life left in the Grinders of Glasses, yet the Warmth of those, that should have used them, has grown cool." No one is using the microscope except for "Diversion and Pastime"; Leeuwenhoek, he says, is almost its unique "votary."[46] Many historians of early microscopy agree with Hooke in noticing this disillusionment with the microscope by the turn of the century.[47] Among the explanations that have been given for this cooling of interest is the intrinsic difficulty of seeing with instruments, especially microscopes. They were not perfect in this sense. Leeuwenhoek had an incredible and somewhat unique knack for microscopic observation, but he was almost the only one who could mount specimens on his awkward microscopes, and see through their tiny lenses. Another explanation might be the very nature of the microscopic discoveries of the time –

[44] Malebranche (1997, 28).
[45] Berkeley (1975, §86); Locke (1975, II.xxiii.12).
[46] Hooke (1726, 261).
[47] Wilson (1995), Fournier (1996), and Ruestow (1996) all concur in the same diagnosis. Eriksen (2018, 25) challenges this view, suggesting that the apparent scientific decline and philosophical disavowal of the microscope at the end of the century coincided with the full absorption of its epistemological lesson. That lesson, however, is not to be looked for in the theory of matter, nor in the scientific disciplines or proto-disciplines dealing with life forms, but in the invention of a new realm of empirical enquiry, the "sub-visible." Eriksen defines it as "the part of nature that lies beneath the scope of natural sight, but above the level of nature's basic units" (12). For a qualified judgment on the supposed decline of microscopy in the eighteenth century, see also Ratcliff (2009).

they indeed revealed a new world, but not one easily integrated with available knowledge, or easily used for answering pending questions in clear-cut disciplines. Contrary to early expectations, the microscope did not reveal a simpler world of easily understood elements, from which one could learn the "true alphabet of nature," but rather an incredible diversity of novel natural productions.

This could open up a vertiginous perspective: each little animal discovered in the pepper-water seemed to have limbs and vessels, into which one could suppose fluids circulating, in which one could imagine other little animals, and so on *ad infinitum*.[48] This indefinite nesting of worlds into worlds may have been good news for physico-theologians, who discerned therein remarkable testimony of the extent of God's providence, or for philosophers, such as Gottfried Wilhelm Leibniz, who saw a confirmation of his views on the infinite internal complexity of the "machines of nature":

> Each portion of matter, can be conceived as a garden full of plants, and as a pond full of fish. But each branch of a plant, each limb of an animal, each drop of its humors, is still another such garden or pond. . . . Thus there is nothing fallow, sterile, or dead in the universe.[49]

But the very same considerations could also nourish a brand of epistemological skepticism, especially toward the kind of corpuscular explanations that mechanical philosophers were aiming at. Peter Anstey coined the phrase "corpuscular pessimism" to express this attitude, exemplified by the young Locke:

> When we goe about to discover the curious artifice of nature and take a view of the instruments by which she works, we may with as much reason expect to have a sight of those very spirits by which we hope to see them, for I believe the one as far from the reach of our senses as the other.[50]

In other words, if the size of the ultimate corpuscles was indeed below the range of *optical* visibility, the Hookean dream of making them an object of visual autopsy had to be given up, and our epistemic expectations drastically reoriented. What was really accessible to our visual experience, however augmented, were various realms, or various levels, of phenomenal properties, which could perhaps be empirically correlated one to another, but never causally explained. Locke later said that a man endowed with "microscope-eyes" allowing him to

[48] For a typical example of this kind of speculation, see Leeuwenhoek's letter of May 20, 1679 (Leeuwenhoek 1939, 3:55).

[49] Leibniz (1991, §64).

[50] Locke, *Anatomia* (c. 1667), quoted in Anstey (2011b, 33).

see (against all probabilities) the ultimate constituents of matter, even though he "could penetrate further than ordinary into the secret composition and radical texture of bodies," would be so cut off from the common world that he could not make any use of such knowledge. "He would not make any great advantage by the change, if such an acute sight would not serve to conduct him to the market and exchange."[51] This pessimistic turn was a symptomatic expression of an uneasiness brought about by the instruments and their alleged modernity. We also find it expressed in Berkeley,[52] and later, in a satiric mode, in Swift's *Gulliver's Travels*. Modern instruments have brought us into strange worlds. They certainly enriched our experience of the universe's profuse diversity, but they also nourished a feeling of estrangement. These new "dioptrical worlds,"[53] on which we furtively glance now, never trodden nor seen by anyone, are not worlds in which, we, as human creatures, are meant to live and pursue happiness and morality.

[51] Locke (1975, II.xxiii.12). [52] Berkeley (1975, §86).

[53] The phrase is from Boyle, who distinguished three worlds: the spiritual, the phenomenal, and the "dioptrical world . . . which consists of all those creatures, that lay conceal'd in former ages from mortal eyes, and are not now discovered without dioptrical glasses" (Boyle 1772b, 5:773–774).

19

Science of Mind

MARTINE PÉCHARMAN

What Rubric for the Science of Mind?

Let me introduce the issue of the science of mind in the seventeenth century by considering Nicolas Malebranche's *The Search After Truth* (1674), which presents the explanation of the nature of mind and of all its faculties as indispensable for "the science of man."[1] The complete study of the human mind, Malebranche says, examines the mental faculties related to the mind's union either with body (sense, imagination, passions) or with God (understanding, will). Yet these faculties are not parts composing the mind. From the outset, the *Search* assumes that mind is immaterial or unextended, and therefore undoubtedly "a simple, indivisible substance without any composition of parts."[2] Thus, the science of mind depends on the real distinction between mind and body. And this, notes Malebranche, "has been known with sufficient clarity for only a few years" – i.e., from René Descartes.[3] In other words, the science of mind, the indispensable condition for "the most necessary of all our knowledge ... the knowledge of ourselves,"[4] was a product of Cartesian metaphysics.

[1] Malebranche (1997, Preface, p. xxxiii).

[2] Malebranche (1997, I.i.1, p. 2). For more on Malebranche's science of the nature of mind, see Schmaltz (1996); Pyle (2003); Jolley (2013).

[3] Malebranche (1997, Preface, p. xl). It was usual among Cartesians (for instance, Arnauld in the Fourth Objections or La Forge in the preface to his *Treatise on the Human Mind*) to attribute to Augustine the essential differentiation between mind and body. But for Malebranche this still required a demonstration that sensible qualities are not included in the idea of matter. On this, compare Descartes's *Principia philosophiae* (*Principles*) II.4 (Descartes 1964–1974, 8-A:42; Descartes 1984–1991, 1:224) with Galileo's *Dialogue concerning the Two Chief World Systems* (Galilei 1967, 309). On Malebranche's view of Cartesians' demonstrations that sensible qualities are modifications of the mind (Malebranche 1997, Elucidation 1.11, p. 634), see Cook (1996); Nolan and Whipple (2006).

[4] Malebranche (1997, Preface, p. xxxix).

This chapter focuses specifically on the incorporation of "the nature of the human mind" (the title of the Second Meditation) into the metaphysical project of Descartes and his successors.[5] I consider the Cartesian tenet that defines the principle of thought not as a mere psychic power, but as a substance distinct from body. This principle, together with its reception in the seventeenth century, marked the rupture of early modern views on mind from the traditional science of the soul.

Before proceeding, though, I should note another, non-metaphysical meaning of the "science of mind," attended by its own early modern polemics. In *The Search After Truth*, Malebranche seeks to use knowledge of the mind's faculties, and the errors they cause, to lay down "a *general method*" for the discovery of truth.[6] But Simon Foucher, in a 1675 dispute with Malebranche, noted an incoherence in this scheme: How could one obtain the science of mind without first possessing the method of the discovery of truth in any science? For Foucher, the method should be antecedent, and constitute an instrument for devising the science of mind and then all other sciences.[7] This critique is interesting inasmuch as it considers Malebranche's methodological project as directed against the use of Aristotelian *logic* in the sciences. Indeed, Malebranche's attempted science of mind came to be viewed as a new logic, on a par with the Port Royal *Logic, or The Art of Thinking* (1662). William Molyneux, for example, claimed that logic had acquired "in the *Ars Cogitandi*, *Recherches* [*sic*] *de la Verite*, &c." a shape quite different from "what it appears in *Smigletius*, and the Commentators on *Aristotle*."[8] For the sake of space, however, I must leave aside this logical aspect of the early modern science of mind.[9]

From Aristotelian Soul to Early Modern Mind

According to Aristotle's *De anima*, the science of the soul (*psuchè*) must determine its essence according to a fundamental hylomorphism. Living natural bodies are compounds of matter and form (*hulè*/*morphè*), potentiality and actuality (*dunamis*/*entelecheia*), and soul is defined as the "first actuality"

[5] I will not discuss whether Descartes's views constitute a *complete* science of mind. See Jolley (2013, 12–27), challenging Nolan and Whipple (2005).
[6] Malebranche (1997, 16–18). "Method" aims "to render the mind as perfect as it can naturally be" (Malebranche 1997, VI.i.1, p. 408).
[7] Foucher (1995, 19–20).
[8] Molyneux (1692, Dedication to the Royal Society). For Molyneux, however, these new logics were surpassed by Locke's *Essay concerning Human Understanding*.
[9] For this second approach, see Corneanu and Vermeir in this volume (ch. 8).

(*entelecheia prôtè*) of a natural body possessing life potentially.[10] On this account, psychology is a part of natural science or physics, specifically the science of the principle through which living things possess life. But this makes psychology the science of a plurality of types of soul. Because life has different meanings – nutrition, local movement, sense perception, thought, or intelligence (*noûs*)[11] – we must view soul as the principle of these four functions. Some ensouled beings possess all, others some, and still others only one. Plants only possess the nutritive faculty; animals add local movement and sensation; humans alone possess the intellective faculty. The hylomorphic definition from which the science of the soul can be constructed accounts therefore for a tripartite soul, according to its vegetative, sensitive, and intellective powers.

De anima's sketch, however, is not limited to this gradation of the "parts" of the soul. Aristotle also raises a difficulty: are all "affections of the soul" shared by the animate body, or is there any "affection" peculiar to the soul itself? If there is such a property, then the soul can be separated from the body; otherwise, its separate existence is impossible. Aristotle highlights that "to think" (*to noein*) seems the best candidate for a separate and incorporeal psychic action. But if it is demonstrated that the intellect (*noûs*) cannot think without the imagination (*phantasia*), then even the intellect is dependent on body and incapable of a separate existence.[12] This ambiguity about the separability of the intellect was the source of the so-called "mind–soul problem" in commentaries on *De anima*.[13] Aristotelians were divided into two interpretive camps. Some viewed the intellect as integrated with the soul's actualization of life in the human body; some saw it as separate from the human soul and subsisting independently from the body. Thus, the commentary tradition was not univocally concerned with the science of the soul as the principle of life. Another vein was noetics, the science of the intellect, apart from the rest of soul and body.

Against this double Aristotelian background, the science of mind was viewed as a Cartesian innovation.[14] Descartes's metaphysics intended to

[10] *De anima* 412a27–28 (Aristotle 1984, 1:656). The Port Royal Logic criticizes this definition as an explanation of something obscure by something still more obscure (Arnauld and Nicole 1996, 128).

[11] *De anima* 413a23 (Aristotle 1984, 1:658).

[12] On this, see *De anima* 403a3–19 (Aristotle 1984, 1:642). For the science of the soul in early modern Aristotelian authors, see Des Chene (2000a).

[13] See Pasnau (2007) for the categorization of this problem and its distinction from the mind–body problem in the Cartesian legacy.

[14] When in 1644 Louis de La Forge composed his *Treatise on the Human Mind* to supply the section on soul and soul–body union missing from Descartes's *Treatise on Man* of the

erase any equivocation. According to the Fifth Replies, Aristotelian psychology, and the notion of the soul's higher noetical part, rested on a merely anthropological differentiation of mind from soul. After confounding under the term 'soul' the principle of biological functions common to all living things with the principle by which humans (alone) think, the earliest humankind, Descartes claims, introduced the term 'mind' to suggest the superiority of thought. The principle of thought should instead be viewed as absolutely heterogeneous. In its new Cartesian use, 'mind' signifies the principle of thought not as a part of the soul, but as the entire soul that thinks.[15] The Geometrical Exposition of the Second Replies illustrates this. 'Mind' is preferred to 'soul' to refer to the thinking substance because 'soul' "is ambiguous and is often applied to something corporeal."[16]

The rejection of the Aristotelian division of the soul's powers was indeed crucial for the early modern redefinition of psychology as the science of mind.[17] Descartes's unpublished *Treatise on Man* ascribes the functions attributed to vegetative and sensitive souls to the motions of a corporeal body-machine, and the principle of life to a continuous heat in the heart.[18] Moreover, in his 1637 defense against Libert Froidmont's accusation that the denial in beasts of "some sort of substantial soul distinct from blood, heat, and spirits" might facilitate atheism, Descartes emphasizes contradictions in the Aristotelian account of the sensitive soul: "when the sensitive soul is alone, its nature is corporeal and mortal; when it is joined to the rational

early 1630s, his explication was modeled on the *Meditations*: "we are indebted to Mr. Descartes alone for having informed us about the nature of the mind" (La Forge 1997, 58). On La Forge, see Manning (2012).

[15] Descartes (1964–1974, 7:356); Descartes (1984–1991, 2:246). For Gassendi's shift from 'soul' (*Anima*) to 'mind' (*Mens*) in his Objections, see Descartes (1964–1974, 7:260–265); Descartes (1984–1991, 2:181–185); Lennon (1993).

[16] Descartes (1964–1974, 7:161); Descartes (1984–1991, 2:114). For the meaning of 'idea' as the form of thought, not a corporeal trace in the brain, see Descartes (1964–1974, 7:160–161); Descartes (1984–1991, 2:113). Descartes's terminological reform was not easily received in early modern lexicons. Compare, e.g., Micraelius (1966, 120–124 [Anima], 629–632 [Intellectus], 750 [Mens]) with Goclenius (1964, 103a–105b, 247a–251a, 681b–682a) for the persistence of Aristotelian definitions. The Cartesian emergence of mind as the "thinking thing" was not acknowledged before Étienne Chauvin's *Lexicon rationale* (1692), expanded as Chauvin (1713). On Chauvin's use of Cally (1683) and Cally (1695) for definitions related to psychology, see Gasparri (2016).

[17] On the early modern revolution about mind, see James (2000); Schmaltz (2006); Garber (1998); McCracken (1998b); Serjeantson (2011). See, for Cartesian psychology, Hatfield (2019); for psychology as a modern discipline, Hatfield (1995).

[18] See Descartes (1964–1974, 6:46); Descartes (1984–1991, 1:134). Only the rational soul survives the Aristotelian typology of souls rejected by Descartes – but see the claim, "no one before me has stated that the rational soul consists solely in thought" (Descartes 1964–1974, 8-B:347; Descartes 1984–1991, 1:296).

soul, it is spiritual and immortal."[19] This opposition is unintelligible, Descartes argues, in a doctrine claiming a distinction of degree, not of kind, between psychic operations in beasts and in humans. Criticizing the Scholastic partition of the human soul into distinct entities, the new psychology requires the unity of the thinking substance. As Descartes writes in 1649,

> there is within us but one soul, and this soul has within it no diversity of parts: the same one which is sensitive is rational . . . . It is an error to identify the different functions of the soul with persons who play different, usually mutually opposed roles – an error which arises simply from our failure to distinguish properly the functions of the soul from those of the body.[20]

The human mind is a simple substance, not a thing composed of a multiplicity of parts, like the human body. Its faculties are not really distinct from it.[21]

In Part Five of the *Discourse on Method* (1637), Descartes offers to demonstrate his view *a posteriori*. He inventories all corporeal functions in order to eliminate the possibility that thought is among them.[22] The demonstration of soul's immateriality thus derives from the limits of materialist mechanism. The Cartesian revolution in the science of mind surely required something more than this argument differentiating humans from beasts or machines. Strikingly, however, the more sophisticated treatment in the *Meditations* (1641) is sometimes dismissed as also arguing only negatively – attributing, in the same way as the *Discourse*, to a (mysterious) non-mechanical principle whatever is not within the domain of a mechanical principle. Descartes's demonstration is then susceptible to the so-called "ghost-in-the-machine" objection made famous by Gilbert Ryle.[23] For Ryle, the Cartesian real distinction between mind and body is a category mistake, rooted in the generic term 'thing' or 'substance'. Because bodies were "things" explicable by mechanical laws, minds were seen as other "things" *non*-explicable by the same laws. In other words, mechanics predetermined the constitution of the science of mind simply through "an obverse vocabulary":

> The workings of mind had to be described by the mere negatives of the specific descriptions given to bodies; they are not in space, they are not

[19] Descartes (1964–1974, 1:415); Descartes (1984–1991, 3:52).
[20] Descartes (1964–1974, 11:364–365); Descartes (1984–1991, 1:346, modified).
[21] See also the Sixth Meditation (Descartes 1964–1974, 7:86; Descartes 1984–1991, 2:59).
[22] See Descartes (1964–1974, 6:57); Descartes (1984–1991, 1:140). As "a universal instrument," reason can belong only to humans; see Chomsky (2009).
[23] On Ryle, see Alanen (2003).

> motions, they are not modifications of matter, they are not accessible to public information. Minds are not bits of clockwork, they are just bits of not-clockwork.[24]

For Ryle, the great metaphysical divide of the mental from the material of which the moderns were so proud yields nothing but an empty "para-mechanical hypothesis" about mind.

Against this, in what follows, I defend the Cartesian "invention of mind" as a rigorous metaphysical account. The ontology of mind was not ready-made by an antecedent "grammar of mechanics," but required patient elaboration. Contemporary critiques helped improve Descartes's positive argumentation, such that he eventually came to a doctrine of substance compatible with the definition of mind in the Second Meditation.

The *Cogito*'s Ontological Commitments

It is important to note, first of all, that the ontological significance of the Second Meditation is not straightforward. By the *cogito* argument – even if I doubt everything material, I cannot doubt my own existence – the doubting I comes to know itself as solely a thinking thing. Yet, Descartes insists, this *ideational* exclusion of body from one's self-knowledge as a mind does not immediately imply the *real* exclusion of body from mind's nature. Indeed, it is only in the Sixth Meditation that the latter exclusion comes as the necessary consequence of the former.[25]

Arguably, however, the intermediary steps add nothing to the content of the premise. The later Meditations do not supplement our knowledge of *what* mind is, and the sixth simply reifies the conception of mind apart from body derived in the second. In other words, the Cartesian science of mind is nothing other than the knowledge of mind's nature as a thinking thing acquired in the Second Meditation. Yet the I's knowledge of itself only as a mind, not as a body, does not preclude the possibility that one and the same

[24] Ryle (1978, 21). See also Rorty (1980), Schmaltz (2006) on the early modern "invention of mind" as a shift from "mind-as-reason" to "mind-as-inner-arena."

[25] Descartes (1964–1974, 7:78); Descartes (1984–1991, 2:54). See also the Preface to the Reader (Descartes 1964–1974, 7:8; Descartes 1984–1991, 2:7). The metaphysical program of the *Meditations* corrects the rashness of the Fourth Part of the *Discourse on Method*, in which two conclusions are immediately derived from the *cogito* argument – I am a *substance* whose nature is solely to think; this I is *entirely distinct from the body* (Descartes 1964–1974, 6:33; Descartes 1984–1991, 1:127). Meditations Three, Four, and Five are intended to make the transition from ideational to real exclusion *become* immediate in the Sixth Meditation.

nature both thinks and occupies space.[26] Thus, even though the Second Meditation does not mistake the mere ideational exclusion of body from mind's nature for its real exclusion, it remains a difficulty, since the self-knowledge resulting from the certainty of the statement "I am, I exist" is still essential to the argument of the Sixth Meditation.[27] This stresses reciprocally that the discovery of the nature of mind in the Second Meditation does not rule ontological commitments out of the *cogito*. What matters, therefore, is to delineate them.

We may begin with the search for the identity of "that I" (*ego ille*) that knows itself as existing. The meditator first looks for possible answers among her former opinions about what she named her 'body' and what she named her 'soul'. The supposition of the malicious demon removes all the attributes included in the body's nature, as well as all the attributes of the "Aristotelian" soul, which cannot exist without the body (nutrition, locomotion, and sensation), but it fails to remove the attribute of thought, understood as the action of thinking. "'To think,' or 'thought,' necessarily survives as the only attribute inseparable from the I, since the I's certainty of existence cannot be distinguished from the actuality of the I's thinking. This marks the oblique introduction of mind through the determination of the I's identity:

> I am, I exist – that is certain. But for how long? For as long as I am thinking. For it could be that were I totally to cease from thinking, I should totally cease to exist . . . . I am, then, in the strict sense only a thing that thinks; that is, I am a mind, or intelligence, or intellect, or reason [*mens, sive animus, sive intellectus, sive ratio*] – words whose meaning I have been ignorant of until now.[28]

A crucial watershed is reached, signifying a total reconfiguration of the medieval science of mind. The term 'mind' and its usual cognates for the first time signify the identity of the I with a thinking thing, the sort of thing that the I is.[29]

Only now can the question of the essence or nature of the I arise: "What am I?" (*Quid sum?*). For this question is now the same as that of the essence or

[26] In the Fourth Objections, Arnauld did not miss the point. For him, Descartes should prove and not simply assume the epistemic sufficiency of the self-knowledge inferred from the *cogito* (Descartes 1964–1974, 7:201; Descartes 1984–1991, 2:141).

[27] The Second Meditation does not contain the inference "I am thinking, therefore I am," but the immediate statement "*Ego sum, Ego existo*." The *cogito* is the I's act of conceiving the proposition 'I exist' whenever producing it.

[28] Descartes (1964–1974, 7:27); Descartes (1984–1991, 2:18).

[29] This turning point also marks a significant change from the way the *Rules for the Direction of the Mind* (circa 1628) challenges the medieval heritage. Descartes's *Rules*, like Bacon's *Novum organum*, rejects the treatment of mental operations in medieval logic or "dialectic." The issue is thus methodological, not metaphysical.

nature of the thinking thing or mind: "What is this [the thinking thing]?" (*Quid est hoc?*).[30] The enumeration of the many modes of thinking answers this interrogation. The nature of the thinking thing or mind is elaborated through its acts: doubting, understanding, affirming, denying, willing, and unwilling; to which the acts of imagining and sensing must be added, even though they cannot occur without the body.[31] Through this enumeration, the intellect loses its medieval privilege – the science of mind cannot just be the medieval noetics – since intellect is only one of the modes of thinking. One task of the new, more comprehensive science of mind will be to differentiate the faculty of knowledge both from lower cognitive powers (sensation and imagination) and from the faculty of judgment and choice (will).

A later step of argumentation elucidates why imagination and sensation are also inseparable from the I. According to the Second Meditation, what can be distinguished only as some mode of thinking is necessarily included in the "I myself" (*ego ipse*), the agent of all acts of thinking.[32] Even supposing that nothing material exists, sensing and imagining have a real existence as "part[s] of my thinking."[33] The dreamer's *sentiment* of colors, sounds, etc. "cannot be false," Descartes claims, and this *consciousness* is a sufficient sign of its reality.[34] This redefinition of sensation and imagination in strictly mental terms sets down a consciousness criterion that finally satisfies the original question of the I's identity. That is, the case of sensation isolates what is common to all the mental acts: the I's being conscious of them.[35] The answer to the question "Who am I?" must be "I am a thinking *or self-conscious* thing."[36]

[30] See Descartes (1964–1974, 7:28); Descartes (1984–1991, 2:19).

[31] Descartes (1964–1974, 7:28); Descartes (1984–1991, 2:19). See also the Third Meditation (Descartes 1964–1974, 7:34; Descartes 1984–1991, 2:24).

[32] See Descartes (1964–1974, 7:28); Descartes (1984–1991, 2:19).

[33] Descartes (1964–1974, 7:29); Descartes (1984–1991, 2:19).

[34] Descartes (1964–1974, 7:29); Descartes (1984–1991, 2:19). For a different reading, see Carriero (2009).

[35] The definition of the essence of mind as self-conscious thought requires accordingly a redefinition of 'idea' as whatever is *immediately perceived* by the mind – no matter the manner of perception – as being *in* the mind. See Descartes (1964–1974, 3:393; 7:160, 181); Descartes (1984–1991, 2:113, 127; 3:185).

[36] On the development of the question '*Quis sim?*', see Descartes (1964–1974, 7:25, 27, 29); Descartes (1984–1991, 2:17, 18, 20). The Appendix to the Second Replies defines 'thought' as "everything that is within us in such a way that we are immediately aware [*conscii*] of it." According to the consciousness criterion of mind, all the operations of will, intellect, imagination, and senses, are "thoughts" (Descartes 1964–1974, 7:160; Descartes 1984–1991, 2:113). See the use of this criterion against Gassendi's objection that since whatever acts exists, I could infer my existence from any one of my actions (Descartes 1964–1974, 7:352; Descartes 1984–1991, 2:244).

This equation of the mind's nature with consciousness grounds the Second Meditation's defense of the thesis that the mind is better known than the body. Against the view (comparable to the "ghost-in-the-machine" objection) that we more distinctly know corporeal things than the I – a mere *nescio quid* not representable in the imagination – Descartes first demonstrates that only "an inspection of the pure mind" (that is of intellect), not sensation or imagination, yields distinct knowledge of some particular body, such as a piece of wax.[37] He then draws a parallel between the wax and the "I myself" to show that any existential judgment about the wax entails that the existence of the "I myself" is known with more distinction. Supposing that that which I judge to be wax is not really wax, or even that I have no senses to perceive anything, "I see, or think I see" suffices to yield certain knowledge not only of my mind's existence, but furthermore of my mind's nature. Accordingly, all my judgments about particular bodies involve self-knowledge of my thinking.[38]

The Cartesian Tetrad

The transition from the *cogito* argument to the definition of mind as a thinking thing self-conscious of any one of its modes involves four main theses about mind, which constitute psychology as a metaphysical science of mind. What I propose to call Descartes's Tetrad about mind can be collected from the above reconstruction of the Second Meditation:

P1. Mind is a thing.
P2. Thought is the essence of mind.
P3. Consciousness is inseparable from the essence of mind.
P4. Mind is better known than body.

Most of the seventeenth-century views on mind were elaborated against one or other of these four assertions. Within the limits of the present chapter, I will only offer extended elaborations of some critiques of P1, but important objections to the remainder are first summarized.

P2, or more precisely the actualist thesis *mind always thinks*, was the target of a refutation by John Locke.[39] Locke refused to transform the mental

[37] See Descartes (1964–1974, 7:29–31); Descartes (1984–1991, 2:20–21).
[38] See Descartes (1964–1974, 7:33); Descartes (1984–1991, 2:22). See also Descartes (1964–1974, 3:394; 7:53, 359–360); Descartes (1984–1991, 2:37, 248–249; 3:394).
[39] Locke (1975, II.i.10–19, pp. 108–116). See Hamou (2018). For the actualist thesis, see Descartes (1964–1974, 3:423, 478–479; 5:193; 7:356); Descartes (1984–1991, 2:246–247; 3:189, 203, 355). See also La Forge (1997, ch. 6).

operation of conscious perception of ideas into the mind's *essence*. Another denial of P2 was Henry More's definition of the incorporeal substance as extended "spirit." More held that indivisibility and penetrability differentiated spirit from corporeal substance, not the property of thought.[40]

For Ralph Cudworth, it was erroneous to combine P2 and P3. As he claims in his famous *Digression concerning the Plastick Life of Nature*, that "cogitation" is the essence of the soul and, in turn, that "clear and express consciousness" is the essence of thought together entail that to cease to have expressly conscious thoughts would be *ipso facto* to cease to exist. Those who hold these positions thus cannot explain states (dreamless sleep, lethargy, apoplexy, embryo in the womb) in which we are "without express conscious cogitations."[41]

Gottfried Wilhelm Leibniz's rehabilitation of substantial forms through the notion of "force" also opened the way to a radical critique of P3.[42] Leibniz held that matter cannot subsist without immaterial substances analogous to souls. These "monads" are all endowed with "perception," the power of internal representation of external things. But not all monads have "apperception," the consciousness of this internal representation.[43] Because Cartesians did not see that perception should be distinguished from apperception and took no account of unconscious perceptions, they wrongly believed that our minds are the only immaterial substances.

As to P4, its critique was linked to its ambiguity. Is it the existence or the essence of the mind that is better known than body? Pierre Gassendi complained that instead of illuminating the science of mind and its difference from the science of body, the Second Meditation remained at the level of truisms and generalities, and bore no new information. The I is a thinking thing, but this was already well known by everybody: "Who doubts that you are thinking? What we are unclear about, what we are looking for, is that inner substance of yours whose property is to think."[44] For Gassendi, the nature of the "internal substance" of the thinking thing remains hidden. The Second Meditation would have made progress had Descartes proceeded to "a kind of chemical investigation" to disclose it.[45]

[40] See More (1966, chs. 27–28); More (1987); Reid (2012).
[41] Cudworth (1977, III.xxxvii.17, pp. 160–161). On consciousness of mental faculties or powers distinguished from consciousness of mental acts, see Descartes (1964–1974, 7:246); Descartes (1984–1991, 2:171–172).
[42] See Leibniz (1989b, 138–145, 213–225).
[43] *Monadology*, art. 14 (Leibniz 1989b, 214). See Anfray (2017).
[44] Descartes (1964–1974, 7:276); Descartes (1984–1991, 2:192–193).
[45] Descartes (1964–1974, 7:277); Descartes (1984–1991, 2:193). See LoLordo (2005); Roux (2008); LoLordo (2019).

The most significant critique of P4 was Malebranche's thesis that we have no knowledge of our soul through an idea.[46] Malebranche contends that we acquire perfect knowledge of bodies by the derivation of all physical properties from extension. However, we do not see our soul as we see a body – by an idea in God that represents it to us. We see it only through consciousness and an inner sentiment. Our knowledge is thus imperfect and defective, "limited to what we sense taking place in us." We cannot know the whole being of our soul, "what it is in itself," or what are "all the properties of which it is capable." So, for Malebranche, the primacy of knowledge of the existence of soul over that of the existence of bodies does not entail a superiority of the knowledge of its nature over the knowledge of the nature of bodies. Notably, Antoine Arnauld devoted three chapters of his *On True and False Ideas* (1683) to refuting Malebranche's rejection of P4.[47]

I now turn to two early modern alternatives to the Cartesian science of mind. Both involve, in different ways, a refusal of P1, which I propose to call Descartes's Realist Thesis about Mind (RTM). The Non-Thing Argument ("Mind is not a thing") rejects both substantial and modal nature for mind. The Other-Thing Argument ("Mind is a non-substantial thing") defends a modal nature for mind. The first completely rejects the RTM, the second qualifies the RTM in non-Cartesian terms. I first illustrate the Non-Thing Argument through a reading of Thomas Hobbes's Third Objections. I will then present two forms of the Other-Thing Argument, in Henricus Regius and in Baruch Spinoza.

The Non-Thing Argument about Mind

Hobbes's argument against the Second Meditation addresses the assertion "I am a thing that thinks, that is, I am a mind."[48] Hobbes endorses the affirmation "I am a thing that thinks," but rejects the equation "that is, I am a mind" that follows. The Cartesian equivalence between I, the thinking thing, and mind cannot stand. This objection is based on Hobbes's idiosyncratic reworking of Aristotle's ontology. A fundamental principle of "all philosophers," he claims, is the "distinction between a subject and its faculties and acts, i.e. between a subject and its properties and essences."[49] The subject of inhering properties or accidents cannot be identical with them, no more than they can be identical with it. It follows that the thinking thing which is the subject of

[46] Malebranche (1997, III.ii.7, p. 237). [47] Arnauld (1990, chs. 21–24).
[48] Mersenne targets a similar claim (Descartes 1964–1974, 7:122; Descartes 1984–1991, 2:87).
[49] Descartes (1964–1974, 7:172); Descartes (1984–1991, 2:122).

thought cannot be the faculties and acts of thinking, which is to say, the mind. The inference "I am thinking, therefore I am thought" is illegitimate, just as "I am walking, therefore I am walk" is plainly absurd.[50] Mind just is thought, so it is a mere property, not a thing. But Hobbes also holds, contrary to the Aristotelian categorization, that 'being' cannot be said in many ways. It is strictly univocal and only applies to the subject in which accidents inhere, not to the accidents themselves.[51] Since mind just consists of accidents, it is not a being at all.

Hobbes's critique of the Second Meditation leads to the conclusion that the thinking thing, as a subject of acts, is material. Hobbes here exploits the Second Meditation's passage about the piece of wax as a corroboration of the Aristotelian doctrine of prime matter.[52] In both, the substratum that remains identical through accidental change is material. Thus, Hobbes argues, "it seems that the subject of any act can be understood only from the standpoint of corporeity, namely, from the standpoint of matter [*sub ratione corporea, sive sub ratione materiae*]."[53] Since our knowledge of the truth of the assertion 'I am thinking' "depends on our inability to separate thought from the matter that is thinking" (its subject), we should conclude that "the thinking thing is material rather than immaterial."[54] In other words, our knowledge of the thinking thing, *qua* thing, must belong to the science of bodies.

Moreover, the necessary correlate of this materiality of the subject is the immateriality of its properties. In Hobbes's view, by transgressing the ontological distinction between a subject and its acts, the Second Meditation surreptitiously attributes to the thinking thing the immateriality of the faculties and acts that mind consists of. Asserting "I am a thinking thing, that is, I am a mind" deprives the subject I of its necessary materiality, and, reciprocally, it produces a pseudo-reification of mind. Mind, insofar as it consists of faculties and acts that are merely inhering properties, is non-material; but its immateriality is not that of an impossible immaterial substance.[55]

[50] See *Metaphysics* 7.1 1028a20–31 (Aristotle 1984, 2:1623) for the ontological distinction between the subject of walking and the act of walking.

[51] See Hobbes (1973, xxvii.1) (stating the dichotomy between *ens* and *accidens*); also Hobbes (1999, viii.1–2).

[52] See *Metaphysics* 7.3 1029a8–26 (Aristotle 1984, 2:1625) on matter as something stripped of all properties.

[53] Descartes (1964–1974, 7:173); Descartes (1984–1991, 2:122, modified).

[54] Descartes (1964–1974, 7:173–174); Descartes (1984–1991, 2:123).

[55] Note that Hobbes's view on mind is thus more sophisticated than its common interpretation. Hobbes does not hold the materiality of mind, but the impossibility of identity of mind with the thinking thing that is its subject. Hobbes's materialist

Hobbes went on to elaborate an alternative science of mind, in which "the mind will be nothing more than motions in some parts of an organic body."[56] In Hobbes's telling, a strictly mechanistic principle explains why an image of sensation, which consists of a motion in the perceiver's body, persists as an image of imagination. Once generated in the perceiver, the motion continues after its stimulus is removed, but gradually decays. The imagination is the "conception remaining, and by little and little decaying from and after the act of sense."[57] Now, new motions are uninterruptedly produced from outside the perceiver, so images of the imagination belong to a continuous sequence, which Hobbes calls the "discourse of the mind."[58] The succession of images constituting mental discourse is therefore predetermined by some original succession in the images of sensation: "The cause of the coherence or consequence of one conception to another, is their first coherence, or consequence at that time when they were produced by sense."[59] This reduction of mind to image sequences means, for Hobbes, a reduction to "nothing more than motions." Our conceptions are mere accidents of the perceiver's corporeal body. Mind, thus, cannot be viewed as a substance, a thing, or a being. The only substance is the body that undergoes the motions of which the mind consists.[60]

The Other-Thing Argument on Mind

Hobbes's (and others') objections occasioned Descartes's mature doctrine of substance, attribute, and mode that eventually appeared in the *Principles of Philosophy* (1644).[61] The response to Hobbes on the Second Meditation's equation of the thinking thing with mind first highlighted that we do not know a substance directly, but only through its acts. For all acts agreeing under "the common concept [*ratio communis*] of thought or perception or consciousness," the Third Replies claims, we impose the name 'mind' on the substance in which they inhere, whereas we call 'body' the underlying

substance monism does not include mind among bodies. See Pécharman (2020). For another reading, see Duncan (2005).

[56] Descartes (1964–1974, 7:179); Descartes (1984–1991, 2:126, modified).

[57] See Hobbes (1994a, I.iii.1).

[58] Hobbes (1994a, I.iv.1).

[59] Hobbes (1994a, I.iv.2).

[60] Insofar as the accidents constituting mind are effects of mechanical causes, the natural powers of mind (before the invention of language) are the object of physics (Hobbes 1999, ch. 25) or of anthropology based on physics (Hobbes 1994a, chs. 1–4; Hobbes 2012, chs. 1–3, 6).

[61] There is very little material for the doctrine of substance in the *Meditations*. See Descartes (1964–1974, 7:44–45, 78); Descartes (1984–1991, 2:30–31, 54); also Descartes (1964–1974, 7:13–14); Descartes (1984–1991, 2:9–10).

subject of acts inconceivable without local extension.[62] This suggested that we have a separate notion of substance as a subject for the inherence of accidents.[63] In the Fourth Replies, however, the thesis of the indirectness of our knowledge of substances gave instead the opportunity to stress that no notion of substance can subsist if we remove all the accidents through which a substance, either the thinking thing or the extended thing, is known.[64]

This move was completed in the *Principles*. Descartes asserts that we can have no knowledge of substance *qua* "existing thing."[65] Our knowledge is only of attributes, and even though any attribute is sufficient to conclude that there exists a substance of which it is a property, we must recognize one "principal property," which is a substance's essence or nature, and is presupposed by all its other properties.[66] Thought has this *constitutive* function for mind, and extension for body. The non-essential attributes of a substance are only *modes*. Rather than the inherence relation of accidents to an underlying subject, this doctrine makes crucial the inconceivability of modes independently of their reference to the principal attribute. Consequently, the list of mental acts (to understand, to will, to doubt, etc.) that, in the Fourth Replies, had entailed the knowledge of the thinking *substance*, now refers to the thinking thing through the necessary intermediary of the principal *attribute*, thought.[67] What has become primordial is the relation of *all* properties to the *one* principal property, which does not really differ from the thing that is their subject. Thought is not a mere property inherent in, or belonging to, the thinking thing; it is "nothing else" but the thinking substance itself.[68]

As noted above, Hobbes would not agree with this ontology, refusing to extend the notion of thing or being to what is not substantial, such as attributes and modes. But Hobbes's was far from the ordinary position, and there was quite another way to critique the Cartesian substantialization of mind.[69] This is the Other-Thing Argument, which hinges on the *thing*-ness of

[62] Descartes (1964–1974, 7:176); Descartes (1984–1991, 2:124).
[63] See also Descartes (1964–1974, 7:161); Descartes (1984–1991, 2:114).
[64] See Descartes (1964–1974, 7:222); Descartes (1984–1991, 2:156). On this change, see Garber (2013a).
[65] *Principles* I.52 (Descartes 1964–1974, 8-A:25; Descartes 1984–1991, 1:210).
[66] *Principles* I.53 (Descartes 1964–1974, 8-A:25; Descartes 1984–1991, 1:210).
[67] Compare *Principles* I.53 with Descartes (1964–1974, 7:233); Descartes (1984–1991, 2:157).
[68] The distinction between the thinking substance and its principal attribute is only a "distinction of reason" (*distinctio rationis*). See *Principles* I.62–63 (Descartes 1964–1974, 8-A:30–31; Descartes 1984–1991, 1:214–215). See also Descartes (1964–1974, 5:221); Descartes (1984–1991, 3:357).
[69] The ordinary position was illustrated, for instance, in Gassendi's counter-objections to the Fifth Replies: the statement 'I am a thinking substance' cannot be substituted (as Descartes does) for the statement 'I am a thinking thing', because 'thing' is not

modes. I here present two very different ways of arguing for the modal nature of mind. The first, which I present through its critique by Descartes, is that of Regius, the second that of Spinoza.

Mind-Mode of Body

Descartes's mature ontological framework imposes a rule of univocal reference: a mode is either of thinking or of extension, it cannot be both mental and corporeal – and, reciprocally, the same thing or substance cannot be the subject of both mental and corporeal modes. This rigorous structure was, however, undermined in a *Programma* published in 1647 by a student of Regius at Utrecht. Presumably composed from excerpts originally intended for Regius's *Fundamenta physices* (1641),[70] the broadsheet was titled *An Account of the Human Mind, or Rational Soul, which Explains What It Is and What It Can Be*. The *Programma* defines the human mind as consisting only of the faculty of thinking, and asserts the possibility that it is "either a substance or a certain mode of the corporeal substance."[71] It contends that the two attributes, thought and extension, which "some philosophers" hold inhere in substances as in subjects, "are not opposites but merely different." Accordingly, the mind can be "a certain attribute appropriate to the same subject as extension." Although mind is not contained in the concept of extension, nor extension in the concept of mind, their common attribution to the same thing is conceivable, so there is no logical incompatibility. This negates the necessity of the mind–body real distinction – a mind *might* be a mode of body. The broadsheet does allow that the modal status of mind can be denied on the basis of Scripture, but not on the basis of metaphysical reasoning.

Descartes, in his *Comments* on the broadsheet, holds that Regius remains stuck on the non-Cartesian notion of attribute.[72] In its ordinary usage, 'attribute' means anything attributed to a thing, whether immutable essence or mode subject to change. But Descartes uses 'attribute' only for the property by which a substance is self-subsisting in its "natural essence." Since a single being cannot have different natures, there is, Descartes claims, no greater opposition between essential attributes than their very difference. To state that there is a difference between attributes is to state that one *is not* the other. Regius errs when he argues that the difference between attributes does not entail their

synonymous with 'substance', but with 'being', and 'being' can be either substance or mode (Gassendi 1962, 116).

[70] See Verbeek (1994).

[71] Descartes (1964–1974, 8-B:342–343); Descartes (1984–1991, 1:294–295).

[72] See Descartes (1964–1974, 8-B:348–351); Descartes (1984–1991, 1:297–299).

opposition. Insofar as extension and thought are not modes but essential attributes, it is impossible that they coexist in a simple subject. Of course, that in which we consider extension and thought together – the human being – is composite, not simple. But Regius, for Descartes, mistakes this composite for a body in which the mind could inhere as a mode.

The polemic with Regius, thus, was about the ontological meaning of 'attribute' and 'mode'. In Regius's formulation, the Other-Thing Argument can be defeated by Descartes's final doctrine on "principal attributes," by which the Second Meditation can be reinterpreted: When I doubt the existence of body, I nevertheless have the idea of a thinking thing. This demonstrates that I have the idea of mind in another way than I have the idea of a mode of body. The necessary conclusion of the ideational test is that the two propositions, "mind is distinct from body" and "mind is nothing other than a mode of body," are incompatible. So, Regius's alternative to RTM is, for Descartes, just bad metaphysics. Descartes could not expect, though, the possibility of quite another alternative to the RTM based on a rigorous metaphysical doctrine about substance, attribute, and mode.

Mind-Idea of Body

At the beginning of Spinoza's *Ethics* (posthumously published in 1677), the Cartesian notion of essential attribute seems to be in the background of the definition of 'attribute' as "what the intellect perceives of a substance as constituting its essence."[73] This definition follows that of 'substance' through self-subsistence and conceptual independence: "what is in itself and is conceived through itself."[74] And it precedes that of 'mode' as a being dependent on substance both ontologically and epistemologically: "that which is in another through which it is also conceived."[75] These three definitions seem comparable to the Cartesian doctrine, but lead to divergent implications for the metaphysical science of mind.

Insofar as an attribute is perceived as constituting the essence of a substance, it shares the epistemological independence of substance. An attribute must be conceived "through itself," *per se* and independently from any other.[76] However, the conceptual distinction between two attributes does not entail that the essences they constitute are the essences of two different substances. In his debate with Regius, Descartes presented the one-to-one relation between essential attribute and substance as the only metaphysical option

[73] Spinoza (1985, 1d4). Spinoza's *Ethics* is here and below cited by part, definition/proposition, etc.
[74] Spinoza (1985, 1d3). [75] Spinoza (1985, 1d5). [76] Spinoza (1985, 1p10).

avoiding absurdity. But for Spinoza, there is no absurdity in attributing to one substance many attributes as constituting its essence.[77] Indeed, a being without a plurality of essential attributes would have a poor amount of being.[78] The basis for the mind–body real distinction thus collapses. It is impossible, from the difference between the two essential attributes of thought and extension, to conclude that mind and body are distinct substances. Instead, we must refer thought and extension to the infinite being, which was defined at the outset as "a substance consisting of an infinity of attributes, of which each one expresses an eternal and infinite essence."[79] All attributes, that is, must be referred to one substance only, the infinite substance called "God."[80]

The consequence of this substance monism (so different from Hobbes's) is an alternative science of mind set against Descartes's RTM. From his substance monism, Spinoza deduces that "an extended thing and a thinking thing are either attributes of God, or affections of God's attributes."[81] He demonstrates the first option. Thought and extension are perceived as each constituting the essence of a substance. Yet we cannot refuse God any constitutive attribute. Thus, thought and extension must be two among the infinity of attributes expressing the nature of God.

What about the "particular things,"[82] minds and bodies? Descartes considered these to be many finite substances. Spinoza, on the contrary, denies the existence of a plurality of substances having the same attribute.[83] Consequently, minds and bodies are nothing more than modes of God's attributes. The divine attributes are infinite expressions of the infinite nature of God, and singular things are in turn determined or finite expressions of them. An individual mind, that is, is a mode that finitely expresses God's attribute of thought; a body is a finite mode of God's extension.

The notion that is central for explaining this relation of expression between the finite mind's modal nature and God is that of *idea*. An idea is defined as "a concept of the mind that the mind forms because it is a thinking thing."[84] Yet the "thinking thing" is not the Cartesian I, because thought is an attribute of God alone, the only thinking substance.[85] As "a thinking thing," God forms

[77] Spinoza (1985, 1p10s).
[78] See Spinoza (1985, 1p9): "The more reality or being each thing has, the more attributes belong to it."
[79] Spinoza (1985, 1d6). [80] Spinoza (1985, 1p14).
[81] Spinoza (1985, 1p14c2). On "extended substance" as "one of God's infinite attributes," see Spinoza (1985, 1p15s).
[82] Spinoza (1985, 1p25c). [83] Spinoza (1985, 1p5). [84] Spinoza (1985, 2d3). See Lord (2010).
[85] Spinoza (1985, 2p1). "Man thinks" (Spinoza 1985, 2a2), but thought is not an attribute of man, because "the being of substance" does not belong to the essence of a human (Spinoza 1985, 2p10).

the idea not only of God's own essence, but also of all that necessarily follows from that essence.[86] Accordingly, "God's idea" represents all the modes of his attributes,[87] those flowing from God's nature as an extended thing among them. It is in this context that Spinoza asserts: "The first thing that constitutes the actual being of a human mind is nothing but the idea of a singular thing which actually exists."[88] The human mind, as a finite mode expressing God's attribute of thought, is then defined as the idea of "a certain mode of extension which actually exists," "the existing body."[89] In other words, every human mind is God's idea of the human's body, arising from God's understanding, *qua* thinking, of God's essence, *qua* extended.

Descartes did not anticipate a refutation of his RTM that would make mind a mode of something other than body. Yet Spinoza's Other-Thing Argument reaches exactly this conclusion. The human mind is itself a *thing* as a mode of the infinite thinking being.

Spinoza's metaphysics had a fascinating consequence for the early modern science of mind. In Spinoza's scheme, "minds" are not just human minds, or even those of animals. There are minds everywhere there are modes of the attribute of extension. Because God has infinite comprehension of all the modes of his extension, God forms ideas of all of them. Thus, we must say that *all* bodies have corresponding minds that are their ideas. Spinoza insists on the generality of his explication of the nature and origin of mind; his demonstration applies no more to humans than to "other individuals, all of which, though in different degrees, are nevertheless animate."[90] This is a most striking application of the Other-Thing Argument, usually labeled Spinoza's "panpsychism." On this view, it becomes important to differentiate human minds from the rest of the ideas representing modes of extension, which is necessary to define the conditions of human knowledge of the physical world.

Another consequence of Spinoza's substance monism is that mental reality and physical reality must be understood as one and the same. Spinoza asserts that, because "the thinking substance and the extended substance are one and the same substance," the idea of a body and the body represented "are one and the same thing, but expressed in two ways."[91] The mind-idea of a body is identical with the body that is its object.[92] Consequently, Ryle's "ghost-in-the-machine" objection cannot apply to Spinoza, who does not *oppose* immaterial substance to material substance, but *identifies* them in God.

[86] Spinoza (1985, 2p3). [87] Spinoza (1985, 2p4). [88] Spinoza (1985, 2p11).
[89] Spinoza (1985, 2p13 , 2p13d). [90] Spinoza (1985, 2p13s). [91] Spinoza (1985, 2p7s).
[92] On puzzles linked to Spinoza on mind, see Lin (2017).

Conclusion

The critical reception of Descartes's thesis that *mind is a thing* outlined two possible alternatives to his substantialist view, either by denying the very *thing*-ness of mind, or by reducing it to a modal being. Yet, the objections I have sketched did not bring back the medieval view of mind as the intellective power of the soul. Undeniably, the Cartesian view marked a decisive break with the *De anima*'s psychological theory and the noetical tradition of the commentaries on it. The Aristotelian way of speaking on soul and the intellect was abandoned. For Descartes, mind was no longer a mere faculty of the soul, but the soul itself as the "thinking thing." From this new ontological perspective, and the later discussion of the mind's *thing*-ness, originated the metaphysical scheme that structured the modern science of mind.

20

Circulation and the New Physiology

GIDEON MANNING

William Harvey's fame derives almost entirely from his demonstration of the circulation of the blood, first publicized in 1628 in *Exercitatio anatomica de motu cordis et sanguinis in animalibus* (henceforth *De motu*) and later defended in his correspondence and *Exercitationes duae anatomicae de circulatione sanguinis ad Joannem Riolanum filium* (1649).[1] From Harvey's point of view, fame was not easily won. In the Epistle Dedicatory to his final work, *Exercitationes de generatione* (1651), Harvey is quoted recalling *De motu* and the "great troubles [*quantum turbarum*]" that his earlier "evening studies" created for him.[2] But as early as 1637, Harvey's discovery was deemed "revolutionary" by the Italian mathematician and astronomer Raffaelo Magiotti, who claimed it would "revolutionize [*rivolger*] all of medicine, just as the invention of the telescope has overthrown all of astronomy, the compass commerce, and artillery all the military arts."[3] Although we can find the occasional dissenter in later years, such as Joseph Browne with his *Lecture of Anatomy against the Circulation of the Blood* (1701), by the eighteenth century Harvey and the circulation had joined the ranks of science's, and especially English science's, great success stories.

Like all revolutions, Harvey's had leaders and foot soldiers, reluctant conscripts, profiteers, victims, and opposing forces, making it a rich historical

[1] For the date of Harvey's commitment to the circulation and the history of *De motu*, see Bylebyl (1973); Bylebyl (1977); Whitteridge (1977). For Harvey's biography, see Keynes (1978).

[2] Harvey (1651). *Exercitationes de generatione* is itself of considerable interest, though it cannot be discussed in detail here. See Meyer (1936); Bayon (1947); Webster (1967b); Lennox (2006b); Ekholm (2008); Goldberg (2013); Deer (2018).

[3] Galilei (1890–1909, 7:65). I follow Bylebyl (1978) in translating '*rivolger*' as 'revolutionize', which Cohen believes might suggest more than Magiotti meant to convey (Cohen 1985, 85–86).

example of scientific revolution.[4] Its eventual success depended on a range of factors, many of which lay beyond Harvey's own work and control. These include the legitimacy of observation and experimental intervention; the difference between variation and pathology; the use of quantification in discovery and proof; the relation between anatomy, natural history, and natural philosophy; printing; political and nationalist concerns; particulars about Europe's Republic of Medicine; and, not least, the people involved. There are many excellent accounts of Harvey's work and the revolution he set in motion.[5] We will proceed here by identifying a series of puzzles, proposed solutions, and the subsequent problems those solutions engendered. By proceeding in this way, we will see what the circulation accomplished, as well as the vibrant research program Harvey's work created.

The next two sections summarize the cardiac and vascular science of the sixteenth century, beginning with Galenic orthodoxy and noting challenges to it. This sets the stage for Harvey's work, which is the subject of the following sections, where the goal is to describe the achievement and novelty of *De motu* by discussing its content and methods, including living dissections, comparative anatomy, a vast amount of anatomical knowledge, and an unusual quantitative argument. The fifth section examines the reception of *De motu* by its most influential French supporter, René Descartes, to show how he uses the fact of the circulation to support his mechanical understanding of the body, indicating how Harvey's revolution ramified into the new science of the seventeenth century. Finally, the chapter concludes with a return to the larger revolution this volume considers and indicates two lessons that we might learn. First, novel facts like the circulation lead to both speculations and experiments in the "scrambles to deflect, co-opt, steal or reinterpret others' claims."[6] Both are vital to the process of scientific change. Second, the success of a scientific revolution must be judged as much by the puzzles it creates for later generations as by those problems it solves more immediately.

[4] I share many of the (by now well-known) concerns with the concept of a scientific revolution. See, e.g., Park and Daston (2006, 13); Heilbron (2013); Wootton (2015). For historiographical insight into Harvey's place in the Scientific Revolution, see Field and James (1993).

[5] Any discussion of Harvey will inevitably include a distillation of the research and interpretation of others and a dialogue with them. I have especially profited from Pagel (1967); Whitteridge (1971); Pagel (1976); Whitteridge (1976); Brown (1977); Bylebyl (1979a); Frank (1980); Bylebyl (1981); Wear (1983); Mowry (1985); Cunningham (1988b); Wear (1990); Porter (1997); Bates (1998a); Bates (1998b); Gregory (2001); Shackelford (2003).

[6] Schuster (2013, 53).

Orthodox Cardiovascular Anatomy and Physiology

Before we can appreciate where Harvey stands, we need to begin with the state of cardiac and vascular science as understood by a typical learned physician in 1600. University medical training was still rooted in disputation and the language and principles of Claudius Galenus's (Galen's) massive corpus, although in a highly interpreted, theoretical, and simplified form. What follows is itself a simplified presentation of the extensive argumentation and empirical demonstrations the typical physician would have learned, ultimately, from reading or being referred to Galen.[7] It must be remembered that Galen's views were extremely comprehensive, related to medical practice, and had been repeatedly confirmed by the ocular demonstration of generations of physicians.

The place to begin is with the veins. Galen believed the veins originate in the liver. In dissection, he could trace the route of food from the mouth to the stomach and intestines, where concoction transforms the food into chyle. Chyle then travels through the mesenteric and portal veins to the liver, where a second concoction transforms the chyle into thick and dark venous or nutritional blood. Venous blood then travels through the hepatic veins, the superior and inferior vena cava, and finally to the rest of the body. Transport is achieved by a sympathetic attraction between the nutritional blood and the parts of the body it feeds. The process is comparable to an inhalation and is sometimes explained by the Aristotelian principle that nature abhors a vacuum. It is also compared to the mysterious and occult phenomenon of magnetism, which was likewise thought to explain the movement of chyle from the intestines to the liver.[8]

The venous blood's movement to the heart and lungs is especially relevant. From the liver, a majority of the blood finds its way to the right auricle or "ear" of the heart (i.e., atrium), through the tricuspid valve and into the right ventricle or "belly" of the heart. Next, the blood travels to the lungs through the "artery-like vein" (pulmonary artery).[9] Galen's idea is that the right ventricle draws the blood from the liver; though, on this account, the right side of the heart is simply a transport stop on the way to the lungs. Upon

[7] For an excellent account of Galen's views and those of his predecessors in Greek antiquity, though oddly focused on explaining "how it was that the ancient Greeks . . . failed to discover the circulation of the blood," see Harris (1973).

[8] Capivaccio (1603, 173). These connections are also made by Galen's critics, such as Isaac Beeckman and especially George Ent in his defense of the circulation (Ent 1641, 28ff.; Beeckman 1939–1953, 3:298).

[9] Galen (1968, 306).

arrival in the lungs, and for that matter wherever the blood arrives at its nutritional destination, it is consumed. There is no question of its returning to the heart or of its originating from anywhere but the liver.

The second vascular system, the arteries, also contain blood.[10] Given that venous blood is made in the liver, however, there is a question of how blood enters the arteries. Galen spent considerable time and effort describing anastomosis, the process through which the veins and arteries directly interact. By the sixteenth century, anastomosis was thought by the vast majority of physicians to only take place in the heart, where a small portion of venous blood passes through pores in the interventricular septum.[11]

But arterial blood is also different in color and thinner than venous blood. What accounts for this difference? The answer is *pneuma*, the life-giving substance or spirit found in the arterial blood linked to the air – that thing we crave when short of breath and without which we quickly die.[12] From the lungs, according to Galen, air travels along the "vein-like artery" (pulmonary vein) to the left auricle and then to the left ventricle.[13] Mixing there with the venous blood, a waste product – fumes (*fuligines* in sixteenth-century Latin) – is produced, the majority of which is drawn back through the *de facto* incontinent mitral valve, the left auricle, and along the pulmonary vein to the lungs, where it is expirated.[14] Only air and fumes, but not blood, are thought to be present in the pulmonary vein.

The arterial blood produced in the left ventricle proceeds first to the aorta and then to the rest of the arteries, where it sustains the life, heat, and motion of the body. Just as the venous blood is pulled from the liver by the right ventricle, the arterial blood is not pushed from the heart, but pulled. Or, just as the pneuma is inhaled into the lungs when we breathe, the arteries "suck up" the arterial blood. And, again, like the venous blood, on reaching its destination the arterial blood is consumed. There is, for Galen, no question of the blood in the arteries returning to the heart.

The heart itself is "the hearthstone and source of the innate heat by which the animal is governed, every part of it is *ipso facto* important."[15] The auricles are

[10] Galen (1984, 135–184).

[11] Galen (1916, 321). This is how Galen was interpreted, for example, by Pietro d'Abano and Girolamo Capivaccio. See Bylebyl and Pagel (1971). Galen also claims that anastomosis can take place between the pulmonary vein and pulmonary artery (leading to blood being present in the former) and at the periphery of the body (Galen 1968, 315–319, 321). This would prove both a support and an obstacle to Harvey's circulation (Bylebyl 1979a).

[12] The Greek πνεύμα (*pneuma*) might be translated as 'breath', 'spirit', 'vital breath', or 'vital spirit'. It was compared to the element of the stars by Aristotle and served as one of the basic principles of Stoic physics. It also figured prominently in Galen's physiology. For discussion of the Greek medical tradition's many uses of *pneuma*, see Nutton (2004).

[13] Galen (1916, 323). [14] Galen (1968, 318, 320). [15] Galen (1968, 292).

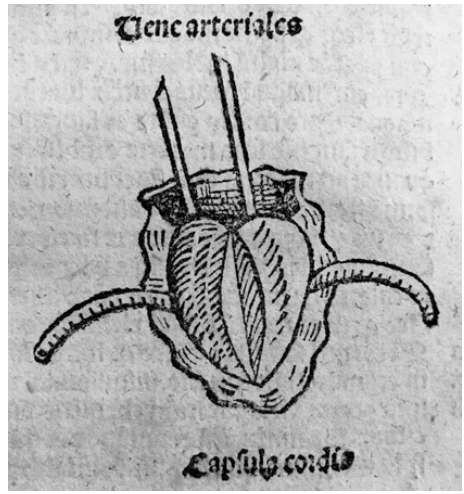

Figure 20.1. Illustration of the two-chambered heart
Source: Hundt (1501). RB 618949, the Huntington Library, San Marino, California

associated with the vessels with which they are linked, the implication being that the heart is a roughly symmetric organ with only two chambers, not the four we think of today (Figure 20.1). Nor is it a muscle, but a unique organ.[16] In our terms, Galen also believes the heart beats during diastole, expanding and striking the chest in the process of drawing in venous blood from the vena cava and air from the lungs.[17] Importantly, the arteries are also active in Galen's account with their own pulsative faculty transmitted from the heart; the heart and arteries each act, inhaling and attracting air and arterial blood, respectively. This means the heartbeat and pulse are simultaneous, with the heart and arteries working in unison, dilating and contracting at the same time. All this motion can be observed, Galen tells us, "in two ways, either by examining [the heart] when it has just been removed from the animal and is still beating," or by "cutting out" the sternum in a live dissection to view the heart directly.[18]

Novel Facts and an Anatomical Insurgency

This, roughly, was the state of cardiovascular science when Harvey was a student. Though Galen's conclusions, methods of discovery, and ocular demonstrations remained essential touchstones, several questions nagged at

[16] Galen (1968, 293). [17] Galen (1968, 294). [18] Galen (1968, 294).

his cardiovascular science. For example, if the blood is made in the liver to be delivered to the whole body, why does the vena cava appear to carry so much nutritional blood to the heart and lungs? If only a small amount of blood travels across the cardiac septum, why is there so much blood in the arteries? Why are the fumes in the left ventricle able to return to the lungs through an incontinent valve when no arterial blood is permitted to travel the same route? In addition to questions like these, greater exposure to Galen's extensive corpus in the sixteenth century meant that textual inconsistencies in his views re-emerged, which made space for at least some diversity of opinion. Finally, Galen's command to see and do for oneself – *autopsia* – it not being "enough to study . . . casually or read . . . only in a book. No, not even in mine,"[19] empowered physicians to see for themselves, and they did not always find what Galen told them they would.

Among those who looked for themselves, we can count the anatomists Andreas Vesalius, Realdo Colombo, and Hieronymus Fabricius ab Aquapendente. Consider the dispute about the structure of the heart and whether there exist holes in the cardiac septum. Few had ever seen these holes. Small openings do exist in the hearts of fetuses between the two auricles – the foramen ovale – but these typically fill with tissue after birth. Andreas Vesalius expressed frustration that he could not find holes in the septum in the first edition of *De humani corporis fabrica libri septem* (1543). In the second edition (1555), Vesalius's frustration had turned to doubt: "I have never come upon even the most obscure passages by which the septum of the heart is traversed."[20] But if no such holes exist, how does blood enter the arteries?

One answer came from Realdo Colombo.[21] His *De re anatomica* (1559) contains a chapter on the heart with an unequivocal rejection of the existence of holes in the cardiac septum: "Between these ventricles the septum is present, through which almost everyone thinks that a passage is open for blood . . . . But they miss the mark [*sed longa errant via*]."[22] He argues instead for the pulmonary transit or circuit; i.e., the movement of venous blood from the right ventricle to the lungs and then back to the left ventricle.

Central to Colombo's argument were living dissections of animals. Opening their thoraces, he found the pulmonary vein filled with blood.

[19] Galen (1956, 3). [20] Whitteridge (1971, 46).

[21] The pulmonary transit was first proposed in the twelfth century by the Arabic physician Ibn al-Nafis and just prior to Colombo by Michael Servetus. Colombo's student Juan Valverde da Hamusco beat his teacher to press in 1556, but acknowledged Colombo's priority. See Moes and O'Malley (1960). For more detail on Colombo's work, see Whitteridge (1971).

[22] Colombo (1559, 177).

Joining this ocular demonstration with elements of the orthodox view, Colombo inferred that blood travels from the lungs back to the heart, completing a circuit in this order: vena cava, right side of the heart, pulmonary artery, lungs, pulmonary vein, left side of the heart, and finally aorta. This implies that air and fumes do not travel along the pulmonary vein unless mixed with blood. Further confirmation of the pulmonary transit came from Colombo's demonstration that the heart's valves are not incontinent and therefore do not allow for the reflux of fumes or anything else.[23] He also used the novel fact of the pulmonary transit to rethink the heart's anatomy and physiology.[24] Thus, properly understood, it is not his inability to see the holes in the septum that leads Colombo to reject their existence. Rather, he rejects their existence because they are unnecessary and without purpose. The pulmonary transit is enough to account for anastomosis.

Colombo also makes a series of objections to Galen's account of cardiac motion that were mostly ignored until Harvey. Relying again on live dissections, Colombo rejects the motion of the heart and arteries as Galen described them. For Colombo, the heart is not active in its languid state, but when it is forcefully expelling blood into the arteries. It is when the heart contracts and turns white that the apex of the heart rises to strike the chest wall, causing the heartbeat.[25] Thus forcing blood into the arteries, the heart causes the arteries to dilate; it is this forced movement that is felt by us as the pulse. The heartbeat and the pulse are still nearly simultaneous, as in Galen, but they occur when the heart is in systole and the arteries are in diastole, which is not Galen's view.

A second novel fact was introduced by one of Harvey's teachers at Padua, Fabricius. Fabricius was the first to find small membranes throughout the larger veins.[26] Called "little doors [*ostiola*]" by Fabricius, the membranes have a "nature like machines [*natura machinata*]" and are akin to "sluices and dams [*septa, & claustra*]."[27] Like engineers' productions, the little doors in the veins serve the use or purpose (*usus*) of holding back the flow of blood to ensure proper distribution.[28] Still thinking with Galen that the venous blood moves away from the liver, Fabricius realized *ostiola* impeded this outward movement but, nevertheless, claimed they do not control the direction of blood

[23] Colombo (1559, 179–180). [24] Colombo (1559, 175–179). [25] Colombo (1559, 257).

[26] Amatus Lusitanus and Giambattista Canano found membranes in some of the veins, the former even calling them *"ostiola,"* but these physicians did not locate the membranes throughout the body, as Fabricius would. The first published illustration of the "little doors" was given by Fabricius's student Salomon Alberti in his *Tres orations* (1585). See McMullen (1995).

[27] Fabricius ab Aquapendente (1603, 4). [28] Fabricius ab Aquapendente (1603, 1, 4).

flow. Instead, he held they slow its movement, turning an overwhelming flow into a drip toward the extremities. In other words, driven by his prior commitment to the direction of flow, Fabricius claims that the *ostiola* were incontinent, just as orthodox cardiovascular physiology held that the mitral valve was incontinent, allowing for the movement of fumes back to the lungs.

Colombo and Fabricius offer examples of modification and assimilation but not revolution. Colombo's reimagining of the heartbeat and pulse combined with the pulmonary transit is not the full circulation of the blood. He continued to believe that the nutritional blood of the veins was different in kind from the blood in the arterial system, and that both were entirely consumed in the tissues.[29] Fabricius, too, did not resist orthodox cardiovascular science, and maintained the belief that the pulmonary vein carried only air to the heart and fumes to the lungs.[30] Galen's physiology was so comprehensive and entrenched in the universities, it was able to accommodate novelty such as the pulmonary transit and venous *ostiola*. Indeed, because Galen's theories were incorporated into the *methodus medendi*, the cost of abandoning Galen completely was extremely high. Even Harvey himself never seemed to want a revolution, at least in medical *practice*, arguing: "the concept of a circuit of the blood does not destroy, but rather advances, traditional medicine."[31]

Harvey's later work transforms Colombo and Fabricius into insurgents. Indeed, Harvey's anatomical lectures give high priority to determining whether Colombo is right that the heart and arteries alternate their dilatation and contraction. And in a conversation recalled by Robert Boyle more than thirty years after Harvey's death, it was the valves in the veins that Harvey specifically credited with leading him to conceive the possibility of the circulation.[32] As Boyle recounts, Harvey began to wonder, if the venous valves do direct the flow of blood toward the heart, where does the venous blood come from? The answer seemed to be the arteries. This, however, is not the story Harvey tells in *De motu*, and it is to the published Harvey we now turn.

[29] For the reception of Colombo's advocacy of the pulmonary transit and the judgment that he was not widely believed before Harvey's *De motu*, see Bayon (1939); Whitteridge (1971).

[30] Fabricius ab Aquapendente (1615, 14–15). [31] Harvey (1993, 98).

[32] Boyle (1688, 157). For discussion, see Bylebyl (1982); McMullen (1995, 494).

De motu: Seventy-two Pages to Revolution

Though unquestionably a masterpiece of succinct and persuasive scientific writing, the genre to which *De motu* belongs – anatomical exercises – was not new, nor was its composition. As Harvey learned during his time in Padua, questions of structure and description (*historia*) and action or operation (*actio* or *operatio*) were the initial topics of proper anatomical study in dissection. First comes observation and dissection to describe the structure of a given part or organ, then the determination of its action or operation, followed by argument about its use or purpose (*usus* or *utilitas*).[33] This, at least, was the ideal, and historians have found precisely this composition in Harvey's work.[34]

The early chapters of *De motu* offer a careful and highly informed critique of orthodox cardiovascular science, focusing on the movement of the heart and arteries. Harvey tells us the heart has four chambers, that it is a muscle expelling blood into the arteries during systole, causing the "heartbeat" and, almost simultaneously, the arterial pulse. In addition, Harvey shows that the blood moves from the vena cava to the aorta through a pulmonary transit. Each of these conclusions is at odds with orthodoxy, though not with Colombo, and Harvey presents far more evidence than ever marshaled before in support of them. Following the injunction of *autopsia*, Harvey adopts an exhaustive strategy of live animal dissection.[35] He reports the dissections of fish, frogs, and other cold-blooded animals, including the removal of the still-beating heart, as well as watching dog and pig hearts slow and eventually stop.[36]

The second half of *De motu* takes up the question of purpose, arguing that the heart's motion – i.e., its active systole – serves the end of circulating the blood.[37] Here was something "so novel [*adeo nova*]" and "hitherto unmentioned":[38] the

[33] The textbook tradition amply attests to the *historia–actio–usus* division of anatomy. For the division in Galen, see Schiefsky (2007). For discussion and references to the early modern period, see Mikkeli (1992, 155–159); De Angelis (2008, 218); Cunningham (2010, 20–24). For translations of Fabricius's comments on the subject matter of anatomy that are very similar to Harvey's definition in his anatomical lectures, see Martin (forthcoming).

[34] Bylebyl (1973). [35] Harvey (1993, 17).

[36] For discussion of vivisection's reappearance in the sixteenth century, see Shotwell (2013). As it relates to Harvey, see Wear (1983). More generally, see French (1999); Guerrini (2003).

[37] Precisely what purpose (*usus*) the *circulation* serves is only tentatively given in Chapter 8 of *De motu* and later in the text, and Harvey remained in search of its purpose to the end of his life (Pagel 1967; Harvey 1993, 92).

[38] Harvey (1993, 45).

circulation of blood involving either anastomosis or transfer of blood through the tissue. In a long sentence that has been subject to a great deal of attention by historians, Harvey expounds the thought process leading him to the circulation:

> Now, as to the quantity there was, I thought particularly long and hard about the results of my experimental animal dissections and opening the veins, an enquiry that took various forms; about the symmetry and size of the ventricles of the heart and of the vessels which enter and leave them (since Nature, who does nothing in vain, would not purposelessly have given these vessels such relatively large size), and also about the elegant and carefully contrived valves and fibres and other structural artistry of the heart; and many other points: and when I meditated even further on the amount, i.e. of transmitted blood, and the very short time it took for its transfer, and I also noticed that the juice of the ingested food could not supply this amount without our having the veins, on the one hand, completely emptied and the arteries, on the other hand, brought to bursting through excessive inthrust of blood, unless the blood somehow flowed back again from the arteries into the veins and returned to the right ventricle of the heart; I then began to wonder whether it had a movement, as it were, in a circle.[39]

This sentence makes clear several things. First, it reveals Harvey's willingness to accept and even seek out final causes operative in the creation of living things. Second, it shows that Harvey's recognition of the size, relative proportion, and structure of the heart – what we might think of as material and formal factors – encouraged him to consider circulation. Nevertheless, third, these factors were not by themselves enough. It was Harvey's quantitative considerations about the amount of blood leaving the heart that led him to the possibility of the circulation (contrary to Boyle's report in 1688, which cites Harvey's reliance on the direction of flow caused by the valves in the veins).

This quantitative "supposition [*suppositum*]" is remarkably easy to appreciate. Harvey measured the total amount of blood that could be found in an animal by exsanguinating sheep, pigs, and other mammals. He then measured the amount of blood the animal's left ventricle could contain. Doing some simple multiplication, if the left ventricle is emptied during systole and we use a conservative estimate of "over a thousand beats in half an hour," the total volume of blood pumped in half an hour would be far greater than what could possibly be created by the liver drawing nutriment from the alimentary

[39] Harvey (1993, 45–46). For a succinct account of the translation issues associated with this sentence and why I have elected to use the translation in Harvey (1993), see Shank (1985, 346).

canal; one simply could not eat enough to account for the amount of blood that passes through the heart.[40] Harvey realized that the blood could not all be consumed on reaching its destination, as Galen believed. The blood in the arteries must travel into the veins at the periphery of the body.

Harvey confirms that this happens using the techniques of the common medical practitioner, who would frequently apply a tourniquet to the arm. When this is done, the veins become engorged and the valves become visible (see Figure 20.2). Harvey adds that blood can only be drawn from such engorged veins on the heart side of the visible valves. Moreover, once the bled vein is empty, it can only fill again from the end opposite the heart – i.e., closer to the hand – because the valves prevent the blood from retreating from the heart side. This process of emptying and refilling is seen in the gap between valve *O* and pressure point *H* in the temporally ordered sequence of four illustrations in *De motu* (the first two of which are reproduced in Figure 20.3). *H* stops the flow of venous blood and there is no reflux through *O*. The gap created between *O* and *H* in such circumstances can only be filled by releasing the pressure at *H*. Furthermore, if the tourniquet is rapidly applied very tightly, the arm even loses its color and veins do not become engorged. Harvey infers that the deeper artery is prevented from transferring blood to the veins in such cases. Thus, concludes Harvey, veins are filled from arteries.

Harvey ends his discussion of his various suppositions in Chapter 14 by reaffirming his answers to the anatomical questions of action (*actio*) and use or purpose (*usus*). The heart's action or operation is to pulse, forcing blood

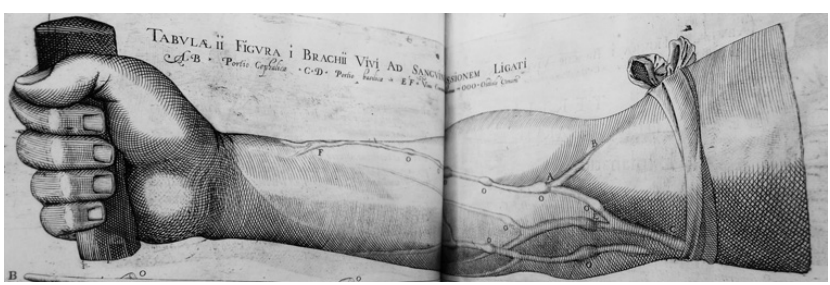

Figure 20.2. Illustration of the engorged veins
Source: Fabricius ab Aquapendente (1603, 10). RB 622128a, the Huntington Library

[40] Harvey (1993, 48–49).

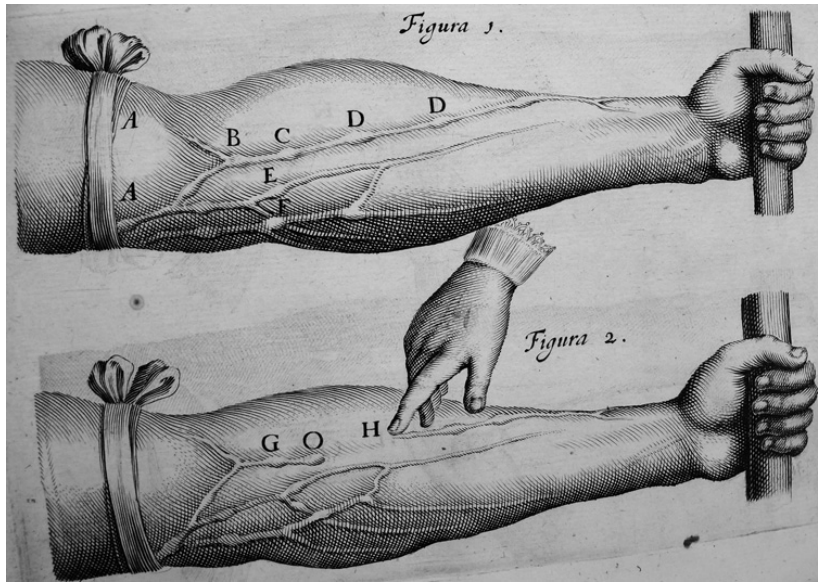

Figure 20.3. Illustration of the engorged veins and the direction of blood flow
Source: Harvey (1639). RB 29612, the Huntington Library

into the arteries. The use or purpose of the heart's movement is to cause the blood to circulate. In Harvey's own words:

> in animals the blood is driven round a circuit with an unceasing, circular sort of movement, that this is an activity or function [*actionem sive functionem*] of the heart which it carries out by virtue of its pulsation, and that in sum it constitutes the sole reason for the heart's pulsatile movement.[41]

Harvey and the New Science

Historians sometimes describe Harvey as a modern scientist in all but name.[42] He is, after all, a magnificent experimenter who finds a use for quantification. Yet Harvey has also been cast as an Aristotelian some distance from a modern scientist.[43] Though this simple contrast is

[41] Harvey (1993, 68). [42] E.g., Whitteridge (1971).

[43] E.g., Lesky (1957). For Harvey's eclectic Aristotelianism, see Pagel (1967); Schmitt (1984b); French (1994); Bates (2000); Fuchs (2001); Sgarbi (2013). See also Frank (1972) and the diverse views of the "Pittsburgh School," including Distelzweig (2014a); Goldberg (2016); Lennox (2017). I generally find Galen (rather than Aristotle) offers a more illuminating source for much of what Harvey actually does. See Kilgour (1957); Bylebyl (1979a); Shank (1985); Nutton (1999).

misleading,[44] Harvey was certainly not engaged in a project of mechanical explanation and reduction of the kind championed later in the seventeenth century (as we will see in the next section). For Harvey, the beating of the heart was a vital phenomenon, caused by a pulsating faculty of the soul, and the blood was itself alive.[45] Though he believed in active systole forcefully expelling blood into the arteries, thereby rejecting attraction as the operative faculty, Harvey continued to hold that the soul caused the heart to beat.[46] He also freely used comparisons between the macrocosm and the microcosm that were anathema to later philosophers of the new science. E.g.: "The animal's heart is the basis of its life, its chief member, the sun of its microcosm; on the heart all its activity depends, from the heart all its liveliness and strength arise."[47]

Further, Harvey's quantitative argument is hardly a rigorous one. He effectively says numbers are irrelevant: "If any transmission of blood occurs in any amount in individual pulsations . . . suppose whatever quantity [you] wish (I am not referring to how much I saw), [and you will be] immediately convinced on this basis by the computed pulsations . . . that the blood goes around."[48] This is not to imply Harvey's quantitative argument is unpersuasive, but it is not a mathematical proof as the geometers or mixed-mathematicians of the age would expect. In other words, we would not be justified in saying that Harvey saw the universe as written in the language of mathematics, as Galileo had, even though they both advised looking to nature and not books when studying the natural world.[49]

Still, the inflection point that *De motu* came to represent is most apparent in the number of problems it bequeathed to those willing to accept the circulation of the blood. It is worth recalling just how comprehensive Galen's cardiovascular science had been. Even though Harvey's initial success was apparent to many of his readers, none could be assured that his methods and conclusions could be used to construct a system as comprehensive as

[44] Wear (1990, 577–580). [45] E.g., Harvey (1993, 72, 104, 131).

[46] There was, of course, a long tradition in medicine of allowing for local mechanical explanations, yet this is a far cry from the project of the seventeenth-century mechanical philosophy (Berryman 2002); compare Erasistratus, on whom see Von Staden (2007). Some have been willing to lay Harvey's circulation at the feet of "mechanical necessities" and "mechanism" (Westfall 1971a, 90; Craver and Darden 2013, 97). For an attempt to clear up the concept of mechanism as it relates to Harvey, see Distelzweig (2016).

[47] Harvey (1993, 3). [48] Harvey (1993, 92).

[49] Precedent for Harvey's quantitative argument can be found in Galen's treatment of urine capacity (Temkin 1961). In spite of its pedigree coming from Galen, Harvey's quantitative argument was stridently attacked by orthodox physicians (Whitteridge 1971, 241).

orthodox cardiovascular science. Harvey and his supporters needed to defend, refine, and extend his demonstrations, a project that became integrated with the new sciences (as we shall see below).[50]

Some difficulties are particularly worth noting. While *De motu* displays that rare ability to design and perform experiments that consistently produce expected results and limit alternative interpretation, Harvey was not insulated from controversies about the legitimacy of experimental interventions as sources of natural knowledge.[51] If anything, the violence inflicted on animals in vivisection posed a special challenge: does one observe the body's normal operation, or its behavior in response to obvious trauma? Even the indirect observation of internal structure in lifeless specimens faced the prospect that the dead body was no longer physiologically the same as the living one. All Harvey could do was insist, as so many others would do, that he had not so "upset and preternaturally disposed" nature to the point of corrupting his investigations.[52] The history of early modern medical experimentation includes sophisticated challenges to, and changes in, the principles of evidence cited to provide "conclusive" testing and discovery. One aspect of Harvey's revolution includes the ongoing refinement of these principles and a growing reliance on living dissections.

There were also many discrete puzzles that came with the acceptance of the circulation, and not everyone agreed about which solutions were crucial. To list some examples: If the heart is a contracting muscle, it must contract continuously all one's life without our willing it – how is this so? What role does the liver now play in the body? How and where does the blood move from arteries to veins? Given that the arterial and venous blood changed places so quickly, how could they be different? What was the connection between respiration and the cardiovascular system? How did the arterial blood fulfill both the nutritive and vital functions? And linked to these last questions, what was the purpose of the circulation itself? As Harvey's initial followers provided answers to questions like these, extending the implications of the circulation and its model to plant life and the function of the nerves, a new physiological tradition took shape. *De motu* was the beginning but not the end of Harvey's revolution.

René Descartes and the Mechanical Circulation

Descartes publicly recognized Harvey's achievement in the *Discours de la méthode* (1637). The acknowledgment comes after a discussion of

[50] For the various and competing ways in which Harvey's work was developed in England, see Frank (1980) and especially Giglioni (2002).

[51] Dear (2006, 112–113). [52] Harvey (1993, 121), cited in Guerrini (2013, 244–245).

Descartes's yet unpublished physics, which included an account of the functions of living things. Therein, he assumes that any given animal movement can have one of two causes, the soul or the body, and he seeks which movements can be explained wholly in bodily terms.[53] Descartes's early list of examples includes digestion, nutrition and growth, respiration, waking and sleeping, sensory experience in all its modalities, memory, hunger and other internal appetites and passions, as well as "appropriate" responses to internal and external stimuli.[54] In the *Discours*, though, Descartes proposes to consider a specific movement "so that you might see how I dealt with this subject." He chooses "the movement of the heart and the arteries," because these movements are, echoing Harvey, "the first and most widespread movement that we observe in animals," from which it follows that explaining them "will readily enable us to decide how we ought to think about all the others."[55] Thus, the movement of the heart and arteries and the circulation of the blood, become an important test – and exemplar – of Descartes's research program, even though they do not define it.[56]

Descartes proceeds to give his own account of cardiac and arterial motion along with a defense and summary of Harvey's demonstration of the circulation from *De motu*, excluding the quantitative argument.[57] Descartes presents Harvey fairly well, though he adds a significant mechanical comparison between the heart and a clock where Harvey would have referred to a pulsating faculty of the heart:

> the movement which I have just explained follows from the mere arrangement [*disposition*] of the parts of the heart (which can be seen with the naked eye), from the heat in the heart (which can be felt with the fingers), and from the nature of the blood (which can be known through observation) [*qu'on peut connoistre par experience*]. This movement follows just as necessarily as the movement of a clock follows from the force, the position and the shape of its counterweights and wheels.[58]

Descartes attributes the activity of the heart to a rapid heating of the blood caused by a "fire without light."[59] Leaving this aside – it is as mysterious to us

[53] Descartes's dualism is not well regarded in the philosophical and medical literature but it creates a great deal of room for mechanistic anatomy and physiology.
[54] Descartes (1964–1974, 11:201–202).
[55] Descartes (1964–1974, 6:46–47); Descartes (1984–1991, 1:134).
[56] Des Chene (2006, 67).
[57] Harvey's quantitative argument assumes that the early chapters of *De motu* correctly describe the motion of the heart and its being completely filled with liquefied blood. By virtue of disagreeing with Harvey on this issue, Descartes cannot accept Harvey's quantitative argument.
[58] Descartes (1964–1974, 6:50); Descartes (1984–1991, 1:136).
[59] Descartes (1964–1974, 6:46–47); Descartes (1984–1991, 1:134–135).

as the faculty Harvey cites to explain the motion of the heart was to Descartes[60] – the rest of Descartes's account works as follows: on ignition, the blood expands, which closes the valves to the auricles and, in the same moment, opens the valves to the arteries into which nearly all the blood in the heart flows. The blood then circulates back to the heart through the veins, replenishing itself in the liver. On its return to the heart the blood mixes with the small amount of heated blood left over from the previous explosion, which leads to a new explosion, repeating the cycle.[61] In this way, Descartes accommodates Harvey's circulation to the so-called "animal-machine doctrine," his comprehensive attempt to introduce mechanism into physiology.[62]

Their shared commitment to the circulation – and their common opposition to those who rejected it – allied Harvey and Descartes. It would nevertheless be an error to tie them too closely together. Their differences over the motion of the heart were significant, as was the philosophical divide between them.[63] Insofar as the animating soul and natural faculties of the Platonic, Aristotelian, and Galenic traditions play no part, Descartes's mechanical account of the heart's movement and circulation of the blood showcases his break from tradition. But the account also breaks with Harvey, who remained very much an Aristotelian on the question of what animates the body and the goals of anatomical study.[64] They also disagreed about the active phase of cardiac function: Descartes, like Galen, supports an active diastole, though caused by the blood's exploding.[65] Beyond the details of their physiologies, Harvey's and Descartes's projects had fundamentally different

[60] Descartes would modify his account in the face of criticism, seeking to compare the fire in the heart to other better-understood phenomena, but at least in published form it remains difficult to understand, as is the heart's near instantaneous and repetitive heating and cooling by blood cooled in the lungs. See, e.g., Descartes (1964–1974, 1:530) and the discussion by Roux in this volume (ch. 4).

[61] Descartes (1964–1974, 6:49–51); Descartes (1984–1991, 1:135–136) and, more fully with modification, Descartes (1964–1974, 11:228–245); Descartes (1998, 172–182).

[62] Descartes's "mechanism" is not straightforward (Descartes 1668, 18). His project is, however, reductive, if not always exhaustively so, and certainly avoids reference to natural faculties or powers of the soul. See Gabbey (2004); Hutchins (2015); Roux (2017); Hattab (2019).

[63] For a range of interpretations on the relation between Harvey and Descartes, see, e.g., Gilson (1930); Toellner (1972); Clarke (1982); Grene (1993); French (1994); Gorham (1994); Duchesneau (1998); Anstey (2000a); Bitbol-Hespériès (2000); Fuchs (2001); Ragland (2016).

[64] E.g., Harvey (1653, ch. 71).

[65] Descartes objected to those who "hold that what I wrote is the same as Harvey's view, simply because I believe in the circulation of the blood; but my explanation of the movement of the heart is radically different from his" (Descartes 1964–1974, 2:501; Descartes 1984–1991, 3:134). For his part, Harvey called Descartes "acute and ingenious"

aims and only slightly overlapping methods. Descartes considers his subject to be *all* animal movement, of which the circulation is just an example; there is nothing quite so expansive in Harvey's surviving publications. And while Descartes performed anatomical experiments, he was not engaged in systematic comparative anatomy like Harvey, who carried out hundreds of living dissections in keeping with the encyclopedic tradition of Aristotelian natural history he learned as a student.[66]

Descartes was not the first or the last early modern figure to use Harvey's account of the circulation for purposes Harvey never envisioned. Still, the circulation of the blood gained a significant partisan in Descartes. It was not universally clear in the 1630s that Harvey's demonstrations and presuppositions, or his reliance on vivisection, would lead to further success. Descartes's public endorsement of the circulation and his later defense of his view of the heart's movement as entirely "mechanical" attracted attention to the circulation, especially on the Continent.[67] Indeed, Descartes's mechanical analogies and his model of explanation, stripped of its excesses, proved lasting contributions. Arguably, they serve even more than the demonstration of the circulation itself to reorient medical thinking away from active faculties and powers, like attraction and the action-at-a-distance it implied. At the same time, the factors entering into Descartes's explanation (configuration, heat, and the nature of the blood) were subjects for experimental confirmation, thus further extending the experimental scope of Harvey's research program. On the other hand, Descartes's retention of elements of orthodox physiology may have softened the blow made by the circulation, allowing more room for its acceptance among physicians.[68] All in all, Descartes's (and others') endorsement of the circulation out of its original context proved essential to Harvey's revolution.

This raises challenging questions for the historian about how to characterize the relative novelty and commitment to tradition in Descartes and Harvey. It certainly does not follow from what has been adduced here that Descartes should be characterized as an apriorist or mere philosopher, or as someone promoting a mechanical program without genuine regard for ocular demonstrations or anatomical details, as some exceptional scholars

but claimed, in effect, that he did not observe the heart's motion carefully enough (Harvey 1993, 137). See French (1989); Fuchs (2001); Ragland (2016).

[66] See Cunningham (1985); Lennox (2006a); Lennox (2006b). The link between natural history and anatomy, and its later uncoupling in the seventeenth century, is discussed in masterful detail in Guerrini (2015).

[67] Van Beverwijk (1644, 118–120); French (1994); Fuchs (2001).

[68] Grene (2005).

have explicitly stated or repeatedly implied.[69] Such judgments may coincide with those of seventeenth-century anatomists we now look upon as harbingers of truth, but changes took place quickly in seventeenth-century anatomy, and respecting this fact, and the fact that Descartes performed anatomical experiments sensitive to their causal implications, we ought to allow a more multi-layered view that does not precipitately praise Harvey and fault Descartes, or vice versa.

Conclusion

If we must choose a single moral to draw from this chapter, beyond the rich example provided by medicine and the life sciences in the Scientific Revolution, it is that the novel fact of the circulation, along with Harvey's anatomical experiments and the arguments he built upon them, inspired both informed speculation as well as further experimentation. Both serve as measures of the success of Harvey's revolution, and they remind us that no revolution in science can proceed with one but not the other. A second moral might be added: for all the problems in orthodox physiology that Harvey's account of the circulation solved, such as the presence of blood in the pulmonary vein or the timing of the heartbeat and pulse, it created a significant number of new problems, including several Harvey explicitly considered but did not resolve to his satisfaction. These new problems are the second consequential mark of Harvey's revolution, creating the vibrant research program that absorbed later figures, many of whom, like Descartes, shared few of Harvey's philosophical commitments or his antipathy to the neoterics,[70] but who nevertheless recognized Harvey as their leader and guide.[71]

[69] E.g., Passmore (1958); Bertoloni Meli (2011, 137); Bertoloni Meli (2013, 55).
[70] Aubrey (1898, 300).
[71] I wish to thank Robert Franks, Mordechai Feingold, and Dr. Feliciano Serrano for many helpful conversations. I am also grateful to the participants at the Iowa conference in 2018, when the initial draft of this chapter was first presented, and especially to David Marshall Miller, for his editorial acumen in slimming down an 18,000-word draft.

21

From Metaphysical Principles to Dynamical Laws

MARIUS STAN

Modern physics depends on dynamical laws; they are the chief components of their respective theories, whose power to explain or predict ultimately comes from them. Generally speaking, dynamical laws are functional dependencies between kinematic and kinetic variables; they are differential equations, relating operators and functions common to a broad variety of motion patterns; and they entail equations of motion – local variants of the laws, adapted to the specifics of some family of systems; e.g., oscillators or elastic gasses. These features are strikingly apparent not just in quantum theory, but also in the modern mechanics of classical processes. Dynamical laws are known as the "cardinal equations" in mechanics that take force as primitive; and as the "central equation" in analytic mechanics.[1] These laws are then customized so as to yield system-specific, particular equations of motion: e.g., the dynamical equations of a rigid body moving around a fixed point; the Navier–Stokes equation for a viscous fluid; and the Euler–Lagrange equation for a forced oscillator.

This fact – namely, that dynamical laws are differential equations whose main role is to entail equations of motion – was not always the case. From its Cartesian birth in the 1640s, classical mechanics was built up from "laws of nature," "laws of motion," or eponymous principles. These early modern laws differed from ours. First, their function was to supply mechanics with a basic *ontology*, or class of unit bodies. The ontology was articulated prior to the laws – usually, as an extension of substance metaphysics – and the laws then were predicated of this ontology of body. Second, these laws supplied *justification* for mechanics – from metaphysics, via the doctrine of body; and from select phenomena of motion, construed as evidence for the laws.

[1] See Gallavotti (1983); Papastavridis (2002).

However, around 1750 the laws of motion begin to lose both functions. They turned into mere assertions of equality between magnitudes, and served *just* to entail (differential) equations of motion for specific mechanical situations. Consequently, in the Enlightenment, the laws no longer made claims about natures, and they became epistemically opaque, in that it was no longer clear what counted as strong evidence for the basic laws of mechanics.

This change is among the critical inflection points where early modern mechanics turned into classical mechanics as we know it. This chapter describes and explains the shift, beginning with an account of how the laws of nature linked mechanics to philosophy in the seventeenth century. Next, I point out an incurable shortcoming in those laws with regard to their role in quantitative theory. Then I explain how the eighteenth century fixed that shortcoming, and how the solutions thereby changed the logical structure of mechanics. I complete my explanation with some key mathematical notions – born in the eighteenth century, as well – that enabled the changed conception of laws of motion. Briefly put, my thesis is that:

(i) the modern concept of laws of motion – *qua* dynamical laws – emerged in eighteenth-century mechanics;
(ii) the driving factor of this reconceptualization was the need to extend mechanics beyond the centroid theories of the late seventeenth century; and
(iii) the development of differential equations enabled the shift.

In addition, I will note how the historical change raised two philosophical difficulties, which *ipso facto* survive in the modern concept of dynamical laws: It is unclear what counted as *evidence* for the laws of motion in the Enlightenment; and it is a mystery whether these laws retain any notion of *causality*. The latter threatens to subvert the early modern dictum that physics is a science of causes.

Throughout the chapter, I am motivated by two aims. One is to strengthen the case that a "quiet" revolution unfolded through the eighteenth century in then-fundamental science.[2] The other is to stimulate research on the foundations of Enlightenment mechanics. We have excellent studies of natural philosophy in the seventeenth century, but little has been done to determine which central themes and research agendas survived into the following century. Conversely, we have some good studies of eighteenth-century mechanics, but not much work on how Enlightenment science related to

[2] This suggestion comes from Caparrini and Fraser (2013); Hepburn (in press).

the philosophy that came before it. I expect my chapter to serve as a specimen of work we could do to bridge the gap between these areas of early modern studies.[3]

Basic Laws in the Seventeenth Century

My case rests on the premise that key seventeenth-century figures believed that ontology and mechanics must be yoked to each other, via the basic laws of motion. The evidence for this is in plain sight; it just needs coherent assembling.

In *Principles of Philosophy*, René Descartes asserts three "laws of nature" after a summary of his metaphysics and epistemology.[4] Of these laws, two jointly amount to a conservation principle (of size, shape, and motion-state) for a single body absent an interaction. The third codifies the result of any two-body interaction by contact: it is always the outcome uniquely compatible with the "least modal change" among the involved bodies.[5] Famously, from these laws he sought – unsuccessfully – to derive the kinematic rules of two-body impact. But the "modes" in question – really, the three effective parameters privileged by Descartes's first two laws – are just the co-essential properties of all extended substances, or bodies. Therefore, his "laws of nature" straddle the ontology of substance and the basic physics of micro-scale processes. They have epistemological import, too. Descartes's evidence for his laws was not empirical. Rather, he rested their truth on alleged facts about the action of God *qua* creator, its simplicity, and that simplicity's conceptual connection to the modes of extended substance.

The failure of "Descartes and others" to derive sound collision rules drove Gottfried Wilhelm Leibniz to complain that the error was in their defective laws of nature – and ultimately in their metaphysics. So he sought to reform the latter by endowing bodies with forces, or powers "inherent in all

[3] In addition to the sources in note 2, important studies of Enlightenment dynamics are Truesdell (1960); Truesdell (1968); Fraser (1983). Verdun (2014) and Calinger (2016) have set the new standard for work on Euler's mathematical physics.

[4] The account is in Book I of *Principles*. There were many criteria for substancehood in the seventeenth century – unity, activity, *per se* conceivability, persistence through change, etc. Descartes's chosen criterion is ontological independence (of anything else). So he infers, correctly, that only God counts as a genuine substance in this sense, and so his metaphysics includes indispensably a philosophical theology. Then he introduces an honorific sense, as it were, of substance *qua* that which needs no other help but God's to exist. *Res extensae* thus count as honorific substances; see Meditation Two.

[5] The phrase comes from Garber (1992), who explains lucidly the true tenor and content of Descartes's third law. For recent discussions of Descartes's laws, and the foundational debates they caused after him, especially in Britain, see Brading (2011); Jalobeanu (2011).

corporeal substance as such." His new ontology yielded "systematic laws of motion," which he placed at the basis of a science of motion that he called "dynamics." Among these laws was that to every action there is a reaction, and that *vis viva* (his name for a species of active force) is conserved in all motion exchanges.

Leibniz's epistemology of laws does not yet seem to have been elucidated fully. Still, it is safe to say, the proof-structure of his famous conservation principle is by *reductio*: unless *vis viva* is conserved, perpetual motion would ensue, which is impossible. As evidence for his law of action and reaction, he used an informal proof by Jakob Hermann, based on the Principle of Sufficient Reason.[6] At any rate, Leibniz did not think that fundamental laws are learned from empirical sources, and his German successors followed him in this commitment. Their disagreements aside, Leibniz thus concurred with Descartes and Nicolas Malebranche that the basic laws of motion fall out of *a priori* metaphysical knowledge.

Few might expect Isaac Newton to have endorsed this heavily philosophical, foundationalist picture of laws. And yet, endorse it he did – at least in metaphysics, inverting the rationalists' reasoning above. The young Newton had an inchoate metaphysics of body, sketched as a conjectural scenario. In it, God singles out finite volumes of absolute space, and endows them with impenetrability, causal agency on senses like ours, and *mobility compatible with the laws of motion*. That is, to be a body requires *inter alia* the power to move as the laws dictate. In effect, Newton at times endorsed a "law-constitutive" account of body.[7] The laws of motion constrain metaphysical theorizing about corporeal substance – and so they are an explanatory bridge from dynamics *to* ontology, not the other way around, as his rationalist peers had it. Still, that Newton expects a bridge between them at all shows him quietly subscribing to the seventeenth-century picture of the two disciplines yoked together by the fundamental laws of motion.

In sum, as governing principles of corporeal nature, in the seventeenth century the laws bridged mechanics and metaphysics; and as statements

[6] See *Specimen dynamicum* (Leibniz 1997, 163), and also his unpublished piece "On Body and Force, against the Cartesians" in Leibniz (1875–1890a, 4:395). His impossibility argument for conservation of *vis viva* is in the posthumous *Dynamica* (Leibniz 1875–1890b, 6:281–514). For Hermann's law of action and reaction, and Leibniz's endorsement, see Stan (2017).

[7] Brading (2011) makes a solid case for this reading, and argues that Newton remained committed to a law-constitutive account of body through his late career, well past *De gravitatione* (where he advanced his conjectural scenario), which is now thought to be a youthful piece; see Biener (2017). See Miller (2009) for an alternative reading of the relation of bodies to laws in Newton.

presumed to be universally true, they had help from a theory of *a priori* knowledge. An insidious problem will bring it all to an end.

Limitations of Early Modern Laws

The current sense of 'mechanics' comes from Leonhard Euler, who introduced it to rename what until then had gone by 'science of motion'. Naturally and tacitly, all took it to describe the motion of *bodies* in general – the ones that populate the world at all scales, from planets to microscopic particles. However, in advertising their theory of motion, early modern authors casually would leave out a crucial fact: bodies are extended, and yet the new science could not handle their motions *qua* extended objects. Specifically, none of the approaches born in the seventeenth century could describe quantitatively how *every point* of an extended body moves from one instant to the next, in response to agencies applied to the body.

Now why they could not handle it – what they lacked in that respect – is an important, difficult question; I give below a partial answer. Still, it is a fact that they were unable to handle the task. As illustration, consider two examples from the zenith of seventeenth-century mechanics. Christiaan Huygens in *Horologium oscillatorium* derived a key result that locates the "center of oscillation" in a compound pendulum.[8] Then Newton in *Principia* (originally modeled after Huygens's approach) derived *inter alia* Johannes Kepler's Area Law that a body moving under a centripetal force around a fixed point sweeps out equal areas in equal times. But note how they both excise corporeal extension out of the account entirely. In Figure 21.1a, the circle *ABC* represents an extended pendulum bob, attached to the rigid rod *FA*. *E* is the gravity center, and *G* is the bob's center of oscillation, whose distance from *A* Huygens derives in his tract. Figure 21.1b presents Newton's derivation of the geometric quantity of the centripetal force on a planet in a Kepler orbit. *P* stands for the mass center of an extended planet, and *S* for the Sun's center.

[8] A compound, or "physical," pendulum is one in which the bob is extended, and attached to a rigid massless rod swinging vertically from a suspension point. In contrast, a simple, or "mathematical," pendulum models the bob as an unextended mass point, attached to an inflexible massless string. The center-of-oscillation problem was to locate in a "physical" bob the point *H* that swings isochronously with an equivalent mathematical pendulum. Specifically, let *P* and *M* be two equal-mass pendulums, compound and simple, respectively; and let *M* oscillate with frequency *w*. The problem is to locate in *P* the point *H* that oscillates with frequency *w*.

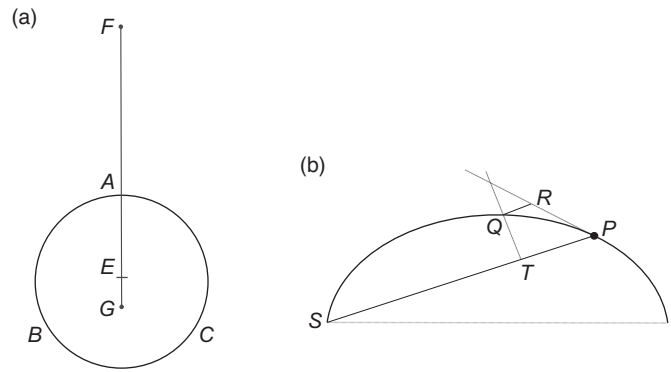

Figure 21.1. Centroid mechanics
Source: (a) Huygens (1673, 121) and (b) Newton (1687, 45)

Though their respective topics (and targets of theory application) are bodies with shape and size, both Huygens and Newton quietly take the "representative point" approach. The pendulum bob above is an extended body, with matter distributed (perhaps unevenly) throughout its volume. Each material point in the bob will move at a different linear speed from its neighbors, but Huygens does not know how to quantify the speed of every point in the bob. So he singles out just one point (the "center of oscillation"), whose motion he *can* derive from his theory. Likewise, the Area Law above was meant for planets and satellites, which – as extended, deformable bodies – engage also in complicated motions, like libration, precession, and tidal locking. But Newton does not know how to quantify the speed of every point in a planet as it performs those motions. So he singles out just one point (the center of gravity), whose motion he *can* derive from his theory.

Eventually the best theorists did grasp the laws' limitation. Euler gave it public expression in a paper on the transmission of force in rigid bodies drafted in the 1730s:

> These principles are of no use in the study of motion, unless the bodies are infinitesimally small, hence *the size of a point* – or at least we can *regard* them as such without much error: which happens when the direction of the soliciting power passes through *the center of gravity* . . . . But if it does not pass through that center, we *cannot* determine the entire effect of these powers. That is all the more so if the moving body is *not free*, viz. it is constrained by some obstacle that depends on its structure.[9]

[9] Euler (1745, §17); my emphasis.

His friend, Daniel Bernoulli, then made the same point in 1745, while emphasizing the insufficiency of Newton's original laws.[10] Put in our terms, Euler's diagnosis was that, in extended bodies and constrained systems, the constraints and the body's other parts alter the motion of any point in the body, but there was no known general principle for quantifying all these kinematic effects.

Mechanics: The Great Inflection

The need for a general treatment – for any possible motion of any possible body – drove theorizing after 1740. Five decades later, it had caused a complete reformulation of mechanics. The locus of this transformation was continental Europe, with Paris and Berlin taking turns leading the way. The Europeans' quest for generality yielded three distinct dynamical laws; each law counted as basic, or fundamental, in the version of mechanics built on it. In this section, I survey them in turn.

Laws of Virtual Work

The most influential strand of new dynamical foundations began modestly in 1703 with Jacob Bernoulli's *Démonstration générale du centre de balancement*, a piece on the compound pendulum, aiming to infer from a safer foundation Huygens's formula giving the center of oscillation.[11] Deep in that five-page paper, he buried the two seminal insights of modern analytic mechanics. The first regarded the statics of rigid weights: Bernoulli stated a generalized Lever Principle. Let *C*, *C*, *D*, and *D* (in Figure 21.2) be masses attached to a bent lever free to oscillate about the fulcrum *A*. Resolve the impressed accelerations into radial and tangential components, respectively. Bernoulli's Lever Principle says the lever is in equilibrium if the sum of each mass times its lever arm times the tangential component (*CB* or *ED*, respectively) adds up to zero.[12] In our terms, the insight was that a system of constrained bodies is in

[10] See Bernoulli (1746).

[11] Huygens derived his formula from an energy principle; i.e., if the constraint (holding the parts of the pendulum bob together) were counterfactually dissolved, the free parts would re-ascend to the same height from which they fell (prior to the constraint being dissolved). In the 1680s, Father Catelan, once Malebranche's private secretary, denied the truth of Huygens's premise. That led some (among them Jacob Bernoulli) to derive Huygens's formula from an incontestable dynamical principle. See also Vilain (2000).

[12] Note that the tangential components are signed magnitudes; e.g., (+) if they tend clockwise, and (−) if counterclockwise.

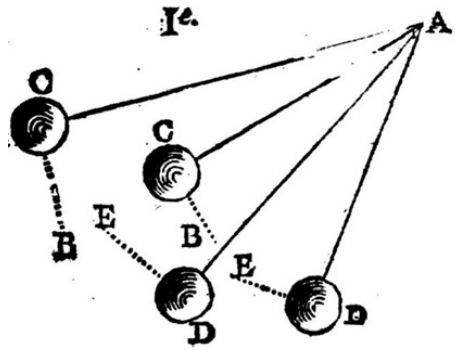

Figure 21.2. Bernoulli's Lever Principle
Source: Bernoulli (1703, 80). Bibliothèque Nationale de France

equilibrium if the virtual work (compatible with the constraints) of the impressed forces is zero.

The second insight extends the virtual-work approach to a system in motion, not at mutual rest (which static equilibrium amounts to). Let some constrained masses $m_{1 \ldots i}$ move under the action of real forces $\mathbf{F}_{1 \ldots k}$. The forces and the constraints jointly cause each mass to undergo a net change in speed $\mathbf{a}_i$; that is, an effective acceleration. The general problem (and main difficulty of analytic mechanics) is to infer these accelerations.[13] Bernoulli's breakthrough was heuristic; he found a way to turn a moving, dynamical system into a static one, which he knew how to treat (from his Lever Principle). He *imagined* that, in addition to the real forces, a set of *fictive* forces also act on the masses, equal and opposite to the products $m_i \cdot \mathbf{a}_i$. (For these fictive forces, we use the terms 'inertial' or 'kinetic reactions'.) Bernoulli's second insight was that, in a moving system, the kinetic reactions balance the impressed forces – if they were real, they would cause equilibrium in the system of masses.

The two insights above became the two basic laws of Joseph-Louis Lagrange's mature theory. After a quiet conversion to virtual-work methods in his youth, Lagrange re-derived all the equations of mechanics then known, from two principles that he stated as equalities.[14] One is for statics, and it says

[13] Here is why it is a problem. If the masses were kinematically *free*, Newton's Second Law would be sufficient: the real acceleration is in the direction of, and proportional to, the net force on every mass: $\mathbf{a}_{real} = \mathbf{F}_{net}/\mathrm{m}_i$. But the constraints prevent the actual-effective accelerations from being as above. Constraint forces are generally unknown, so we cannot use the Second Law to compute their individual effects. That is why a different law is needed.

[14] Fraser (1983) is an excellent overview of his first theory of mechanics, with an outline of D'Alembert's place in Lagrange's decision to move beyond it.

that a system of bodies remains in equilibrium if the virtual work done by the system is zero. The other is for dynamics, and it asserts that, in a system of bodies, the net virtual work of the real, impressed forces and of the fictive, added-in-thought "kinetic reactions" vanishes. Lagrange does not spend much time stating his fundamental principle in words. Rather, he moves quickly to express it as a second-order, ordinary differential equation.[15] This formula allows Lagrange to derive the equations of motion for every point in a system of interacting bodies, whether free or subject to holonomic constraints.

For historiography, this version of mechanics holds a special interest. With Jean le Rond D'Alembert's 1743 treatise, the virtual-work approach became the canonical presentation of mechanics for the century or so in which France was the world's powerhouse in that discipline.[16] In contrast, the Newtonian approach (outlined next) was and remained the preferred version for a minority of one: Euler.

Balance Laws

The same generic blind spot of seventeenth-century theories (the motion of extended bodies under constraints) drove the rise of another formulation. It began with Euler's quest, throughout the 1740s, to obtain equations of motion for a volume element (dV) of an inviscid, incompressible fluid; for a point-mass bound to a rigid surface; and for a rigid body moving around a fixed point. By 1758, Euler had come to think that one expression was enough to entail equations of motion for most mechanical systems regarded then as paradigmatic. Euler called his formula a "new principle of mechanics," and wrote it as:

$$\mathbf{f} = md^2\boldsymbol{x}/dt^2 \tag{1}$$

That is, for every point in an extended body, the net force $\mathbf{f}$ equals the mass (at that point) times its resultant acceleration. Euler's principle functions as dynamical law in our sense; i.e., it entails equations of motion for several classes of extended bodies or systems.[17]

[15] Lagrange (1788, 195).

[16] Lagrange's principles of virtual work above remained the dynamical law for his greatly expanded second edition of *Analytic Mechanics* (1811–1813), for Laplace's five-volume treatise on celestial mechanics (1796–1825), and for Poisson's international bestseller, *Physical Mechanics* (1833).

[17] To see some of the equations he obtained from it, see (for the rigid body) Stan (in press); (for fluids without drag) Darrigol and Frisch (2008); and (for applications to celestial bodies) Verdun (2014).

Subsequently, Euler discovered that his law (equation 1) was not quite general. By itself, it was insufficient for deriving all the equations of motion needed. Specifically, for some systems (e.g., thin elastic rods, plates, and shells) the above law is not enough. Another principle must be added to it:

$$\mathbf{t} = d^2\mathbf{l}/dt^2 \tag{2}$$

In words, the net torque on every point in a system of bodies equals the change in angular momentum by the system at every instant. Together, as he came to see, the equality formulas (1) and (2) are jointly sufficient for the generic task of deriving all the equations of motion.

After Euler, these two laws went dormant until the 1830s, when Augustin-Louis Cauchy and Claude-Louis Navier resurrected them for their mechanics of elastic bodies and viscous fluids.[18] Nowadays we know them as the Balance Laws of Linear and Angular Momentum, respectively, because they relate the force and torque to the net change in those two kinematic quantities.

Extremal Laws

One version of mechanics grew into maturity mostly unnoticed; its birth sparked much enmity and extravagant theologizing, which distracted many from grasping how this version rapidly grew into a serious contender for the title of general dynamics.

This approach arose in the mid-1740s, when Pierre Louis Maupertuis found a way to derive two known results – the equilibrium condition for a straight lever and the kinematic equations for two-body impact – from a new law, which he called the Principle of Least Action. Around the same time, Euler used this principle for the harder task of deriving the equation of motion for a particle in Keplerian motion around a fixed center of inverse-square attraction. Euler also devised a geometric version of the new calculus of variations needed to extend Maupertuis's new law. By 1753, he also had discovered that in certain systems, the action is a maximum, not a minimum.[19]

Still, this was far from general, and it was anyone's guess just how much heuristic potential the least-action approach had. Though he was then just a youth toiling in obscurity at Turin, in 1760 Lagrange answered the question magisterially, in two steps. One step was a pithy version of the calculus of variations, restated in "analytic" (i.e., non-geometric) form. The other was to

[18] For that reason, some authors – e.g., Truesdell (1991) and his disciples – call them "Cauchy's Laws of Motion" to honor the French mathematician who showed them to be properly general.

[19] For details on Maupertuis, Euler, and their context, see Pulte (1989).

apply his calculus "to solving different problems in dynamics."[20] Specifically, he used it to state a "general principle," by reformulating the older Principle of Least Action as follows:

$$\delta \int (m_i \mathbf{v}_i) ds = 0 \tag{3}$$

From this variational principle, Lagrange then derived equations of motion for some eight different dynamical systems.

The upshot of these rapid developments is that, by 1790, mechanics had witnessed a proliferation of basic laws, competing for supremacy over the principal task of theory; and that these new laws were all stated as differential equations, wholly unlike the seventeenth-century laws of motion.

Enabling Factors: New Mathematical Structures

Change in the aims of theory-building after 1740 cannot by itself explain the deep transformation that laws of nature undergo in the Enlightenment. There was another development, and I include it here as a co-explanans. It was the birth of four new notions in mathematics, specifically in the "new analysis," as they called it then. These notions jointly amounted to a result that helped change the logical structure of mechanics beyond anything its early fathers would have recognized. Here I survey these four concepts, and explain their relevance.

The first was the notion of a *variable*, then defined by appeal to an associated concept, that of a fixed quantity or constant value: "A variable quantity is a quantity that is indeterminate, or universal, and comprises every determinate value. . . . Thus a variable comprises in itself every number, be it positive or negative, integer or fraction, rational, irrational, or transcendent."[21] In practice, this notion behaved very much like our modern concept of a variable ranging over real numbers.[22]

The second discovery (an immense breakthrough in early modern science generally) was the idea of a *function*. Several function-concepts emerged in the early Enlightenment, but the most important for my case was this one: "a

[20] Lagrange (1762, 197). [21] Euler (1748, 4).

[22] Of course, scholars of this period did not have the notion of real numbers, or that it is a well-ordered set. What they had were practical equivalents: the notions of integer, rational fraction of integers, magnitudes representable by Euclidean construction (i.e., irrationals), and also "transcendent" quantities, understood then as numbers that cannot be so constructed.

function of a variable quantity is an analytic expression composed in any way whatsoever from that variable and numbers, or constant quantities."[23] To explicate for the modern reader, an "analytic expression" is a combination of variables and constants, related by the operation-symbols of classical algebra (+, −, ×, ÷, and √). For instance, a function f of x (i.e., fx) might be expressed as $\sqrt{(9-x^2)}$.

The third advance was a reconceptualization of the essential notions of calculus, the derivative and the integral. Leibniz and his early disciples had conceived of both as magnitudes: dx as an infinitely small increment in some variable quantity x; and the (definite) integral as a *summatrix*, or infinite-addition ∫ of such increments. At mid-century, Euler reformulated these notions as two operators relating functions. In particular, for him the derivative was a function, g, that stands to a given function f in the relation

$$\mathrm{g} = d\mathrm{f}$$

Conversely, Euler redefined the integral as the anti-derivative; i.e., as the function v that stands to a given function, w, in the relation

$$\mathrm{v} = \int \mathrm{w}$$

A further and equally decisive innovation was Lagrange's extension of the calculus. He took an existing set of techniques (for solving so-called "isoperimetric" problems by geometric approaches) and turned it into the core of a general theory formulated algebraically (not geometrically, as variational problems and techniques had been stated before him). Specifically, he created a new differential operator (the so-called variational δ) that he defined purely algebraically, in terms of its commutation rules with d and ∫, which he likewise reconceived as operators on functions.[24] For mechanics, Lagrange's innovation was an enormous step forward, since it allowed him to formulate the laws nowadays called the "variational principles" of classical mechanics. Both men were explicit about the ways in which they had set the Leibnizian calculus on new foundations and extended it.[25]

[23] Euler (1748, 4).

[24] Lagrange's conception of d and ∫ as operators on functions was in line with his broader program of "analytic mathematics," i.e., of taking algebra (*qua* theory of operations on magnitudes) to be the general framework of *all* mathematics, with (analytic) geometry, arithmetic, and the calculus as species of this algebraic formalism. For lucid explanations, see Fraser (1990).

[25] Lagrange (1788, 196); Euler (1824, 4).

The fourth innovation required the three above. Conceived as an "analytic expression" in the sense above, a function denotes a quantity, "so a function of a variable is itself a variable quantity."[26] That is, a function just is a quantity – with the analytic expression making explicit the exact nature of its dependency on the underlying variable.[27] This view of functions then enabled mathematicians to radically expand the concept of an equation. By 1750, they had come to allow that, in an equation, the unknown variable can be a function – since a function always denotes a variable quantity. This new object is a "functional equation." Even more importantly, mathematicians allowed – by analogous reasoning – that differential operators *on functions* can appear in equations so reconceived:

> To explain this matter, let y be some function of x, and let it be defined by the quadratic equation $y^2+Py+Q=0$ . . . where P and Q are functions of x . . . . In the same way as the finite equation $y^2+Py+Q=0$ displays a relation between y and x, likewise a differential equation expresses a relation, or ratio, that dy and dx have to each other.[28]

That was the birth of differential equations as we think of them.

A qualification is needed. There were avatars of differential equations before 1740. For instance, Newton's "fluxional" equations, which required a "fluent," or variable, to be inferred from its "fluxion," or derivative, *qua* a solution to the equation. But Newton's idea had a critical shortcoming: it was inextricably bound to a geometric configuration. (His fluent was the possible orbit of a point-particle, and the fluxion was its instantaneous velocity.) In contrast, the Leibnizians' mathematical innovations above made it possible to study a differential equation as a self-standing object – unbound and independent from any geometric-kinematic representations it may have, such as orbits, worldlines, or families of curves traversable by matter points.

Here is how these innovations matter. Regarded from the vantage point of its mathematical structure, mechanics stops being a science of geometric representations (of motion patterns at specific scales); and it turns into a *taxonomic system of differential equations*, connected by genus–species relations. All versions of late-Enlightenment mechanics feature a generic differential expression, holding true of any mechanical system, irrespective of its geometry or mass/force distributions; and many specific expressions, one for each

[26] Euler (1824, 5).

[27] That is, the thought we convey by writing '$f(x)=y$'. We regard y as the dependent variable, with x counting as independent relative to y. Then we specify their functional dependence by writing, e.g., that $f(x)=x^2-5\sqrt{x}$.

[28] Euler (1755, 241–242).

particular type of system. A paradigm example of the former is the Euler–Lagrange equation; and of the latter, the equation of motion for a binary star, a vibrating membrane, or a precessing gyroscope, and the equilibrium condition for a load-bearing truss, a capillary surface, or a buckling column.

It is easy for us moderns to miss the explanatory value of this particular development. It appears as a merely formal stage in the evolution of pure mathematics, with no obvious relevance to the science of nature. But that is a distorting effect of hindsight. The makers of mechanics did not see a clean-cut division of labor between pure and applied mathematics, as we do. Quite the opposite – many of their key results in now-pure mathematics were driven by problems and urgencies in mechanics.

Academic Foundations

This far-reaching change in the status of the laws of motion occurred below the surface, as a deep undercurrent within cutting-edge theory construction carried out by leading mathematicians. My picture would not be complete, however, without a brief account of how some philosophers attentive to science saw matters.

In general, philosophers missed the radical change happening at the time. The rapid increase in the formal complexity of mechanics, coupled with the philosophers' physical separation from the sites of theory construction, may well have contributed to the alienation of the two communities, philosophers and working mathematicians.[29] Perhaps as a result of this estrangement, the philosophers adhered to, and reinforced with novel doctrines, the seventeenth-century notion of laws as metaphysical principles. Specifically, they restated the rationalists' two tenets above – i.e., that the basic laws of mechanics are corollaries of substance metaphysics, and that their truth rests on evidence from philosophy, not from empirical research.

Consider Christian Wolff, who was explicit that, in the science of motion, basic laws are the proper business of the philosopher:

[29] Wolff and Kant worked in academia (at Halle and Königsberg, respectively) far from the Royal Academy of Berlin, the research institute where Maupertuis, Euler, and Lagrange worked. So they could not attend the presentation of mathematical work in progress, nor the ensuing discussions. And they lacked the skills to profit from the theorists' work; before the birth of the École Polytechnique in 1792, training in advanced mathematics was available only through membership in informal networks of coaching and personal apprenticeship. (E.g., Jacob Bernoulli in Basle taught Jakob Hermann; then his brother, Johann, trained Euler and Maupertuis. D'Alembert and the young Lagrange were self-taught.) Émilie Du Châtelet could well have come to learn and think about the foundations of Enlightenment mechanics, but she died young.

> The general principles of the rules of motion are called the *laws* of motion . . . . These days only a stranger to Mathematics is unaware that in the rules of motion there are *general* principles, from which these rules can be derived. These principles once established, the rules of motion, i.e., of impact, were proved from them in several ways. Mathematicians assume these *laws* without proof; but it behooves the Metaphysician to *demonstrate* them.[30]

Then he goes about his task. In *Ontologia*, he articulates – from mere concepts, with no empirical input – a vast theory of "simple" and "composite being." Then he applies it to bodies, in *Cosmologia generalis*, as follows. Bodies are composite, he argues, and *eo ipso* an ontological species of composite beings. Second, all bodies have – or at least mechanics takes them to have – three essential properties: extension, active force, and a passive force of inertia. Wolff theorizes from a strong commitment to the Principle of Sufficient Reason, and so here he sets out to uncover the sufficient reason for the three essential properties, which he finds in the attributes of certain ontological ultimates that he calls synonymously "simple beings," "elements of bodies," or "physical monads." Bodies are aggregates of them. In turn, bodies *qua* aggregates are the subject-matter of two "laws of motion," namely a law of inertia and a law of action–reaction. Both laws are about two essential properties of body, active and passive force (of inertia). That is how ontology links up with mechanics in Wolff's doctrine – via his laws.

Immanuel Kant is the last heir to this foundationalist picture of laws, his idiosyncratic jargon notwithstanding. This emerges in his 1786 tract, *Metaphysical Foundations of Natural Science*. The genus-concept of Kant's ontology is "object of possible experience," which he divides into two species, according to the two perceptual channels ("inner" and "outer sense") whereby we access these objects. Any object of outer sense counts as matter, for Kant, and so it inherits all the topological, metric, and affine properties of space, insofar as it is the form of outer sense (which he thinks he has established in the Transcendental Aesthetic).[31] Further non-empirical inferences – including an argument that motion is an attribute of *all* matter – result in a set of theses that he calls the "general doctrine of body," "metaphysics of material nature," and also "metaphysical foundation of natural science." Among these theses are three "mechanical laws": the conservation of mass, a law of inertia, and a principle of action–reaction.

[30] Wolff (1731, §303).

[31] Kant takes himself to have established these results in the Transcendental Exposition of the Concept of Space, at the end of the Transcendental Aesthetic in the *Critique of Pure Reason* (Kant 1962–1963, B40–42).

> First mechanical law: In all changes of corporeal nature, the total quantity of matter remains the same, without increasing or decreasing. . . . Second mechanical law: All change in matter has an external cause (every body continues in its state of rest or motion in a straight line at the same speed, unless it relinquishes this state, compelled by an external cause). . . . Third mechanical law: in all communication of motion, action and counter-action are always equal.[32]

But Kant's confident talk above runs into the same problem as his two predecessors, Wolff and Émilie Du Châtelet. Specifically, all three use the vocabulary of legislation – *leges motus*, *lois du mouvement*, and *mechanische Gesetze* – but they leave unclear what these laws are *for*; i.e., what they do within mechanical theory. More regrettably, Kant, the only philosopher to live through the great transformation in the laws' form and function described above, missed the chance to notice and reflect on it – an unfortunate victim of his deficient schooling.

And so a keen observer around 1790 would have witnessed a paradoxical outcome: as mechanics became ever more able to handle real, extended bodies, it ceased to be a theory overtly about bodies. Kant had put forward his book as a "general doctrine of body," yet paid no attention to how it might connect with the most general, cutting-edge science of moving bodies – Lagrange's mechanics. The latter, in turn, had just published *Mechanique analitique*, a tract conspicuous for the absence of any explicit notion of body, even though it is a book about their motion, and their nature had long been a focus of philosophical inquiry in the two decades he had spent in Germany.

Residual Problems

Beyond the unresolved paradox just mentioned, there are two more foundational worries induced by the silent metamorphosis of basic laws in the eighteenth century. I outline them here, in the hope that future research will address them.

Causation

By relying on the same mathematical formalism, the basic laws of Enlightenment mechanics came to share an important trait. Specifically, the laws above merely assert *equalities between magnitudes*, or that some

[32] Kant (1911, 541–545).

quantity equals an algebraic combination of other quantities. That is all the more so as the laws emerged in response to the same need for equations of motion, despite their outward dissimilarity.

However, assertions of equality are neither material implications, nor if–then statements, nor any of the logical-syntactic vehicles used to predicate cause-and-effect links or dependencies. And so the basic laws no longer count as *causal* principles after 1790; nor do their local corollaries, the equations of motion. In effect, the laws no longer state facts about the basic causal powers of bodies, material substances, or their ontological analogues. If late-Enlightenment mechanics is still home to causal knowledge, it is not contained in the laws of that discipline.

This astonishing consequence – it really heralds the evanescence of causality in fundamental science – remained unnoticed until Russell came across it.[33] Disappointingly for the modern reader, it eluded the eighteenth century's great minds. Oblivious to the tectonic shift beneath their feet, philosophers and reflective men of science alike continued to refer to physics, whose rock bottom was then mechanics, as the "science of causes." But a hard-headed interlocutor might have wondered where causes were hidden, seeing as they no longer resided in the laws.

Confirmation

The deep shift in the nature of laws had long-term epistemological consequences, too. Specifically, it is not clear what counted as *evidence* for basic laws any more. This grave problem – that laws lacked even *prima facie* warrant – had two causes.

First, the standard seventeenth-century sources of evidence for laws are no longer available to eighteenth-century mechanics. Top-down approaches to justification are now beside the point. As the laws no longer mention bodies or their causal actions, potential rationalist evidence (from substance metaphysics and philosophical theology) is irrelevant to the truth of the *dynamical* laws in the Enlightenment.[34] Yet the bottom-up approach, which Newton's work epitomized, becomes out of reach. Here is why: the *Principia* treated just a class of phenomena – motions at celestial scales – for which Newton's

[33] Russell (1912).

[34] Caution: my points in this section pertain only to the law-statements above. As I explained, the philosophers worked with a different set of statements (which they kept calling laws). Their laws *were* amenable to top-down justification, and so of course much of their work in philosophy of physics aimed to obtain just this sort of *a priori* warrant.

epistemic situation was privileged in two senses: he had vast amounts of data (going back to Babylonian astronomy) that informed him reliably about the effective parameters of celestial motions and their key values; and the solar system (the main object of his theory) is remarkably well-behaved compared to most species of mechanical system at terrestrial scales.[35]

Second, Enlightenment theorists were severely disadvantaged in both the respects that had favored Newton's search for evidence. For one thing, they had *no* reliable data with which to confront their equations of motion (so as to thereby extract evidence for the laws that entail the equations).[36] For another, they were evidentially thwarted by the inexorable presence of large secondary effects, often due to agencies beyond the descriptive reach of the very theory in which the equation of motion belonged.[37]

In retrospect, we see that an important question – about the *truth* of the post-Newtonians' dynamical laws – soon became an enigma, avoided until late modernity.

Conclusions

I have argued that our concept of laws of motion – namely, as dynamical laws – emerges in eighteenth-century mechanics. The chief driving factor for this shift was the need to extend mechanics beyond late-seventeenth-century centroid theories. For this shift, the key enabling result was a development in mathematics, the rise of differential equations.

[35] For Newton's investigation, gravity is the only dominant source of motion; and when its secondary effects become relevant (e.g., in Saturn's attraction on Jupiter, in *addition* to the primary attraction on it by the Sun), Newton found a way to handle it by relying on perturbed-orbit methods, as in Book I, Section XI of the *Principia*. Moreover, in gravitational set-ups, deformation rates are very slow, and so deformed configurations (e.g., the Earth's polar flattening) can be treated as quasi-static – hence Newton did not need an equation of motion for every part of the Earth (as an extended body) moving relative to the others under gravity and centrifugal force.

[36] The equations of eighteenth-century mechanics describe infinitesimal deformations and instantaneous velocities at a point (inside an extended mechanical system). But measuring values for such local parameters requires very advanced technology – like high-resolution microscopy, fluorescent molecular doping, wind tunnels, and high-frame-rate cameras – not available until late-Victorian physics and beyond. For instance, the earliest empirical data in elasticity theory (by Chladny, on the vibration of membranes) gets taken up by theorists after 1800.

[37] E.g., elastic media available for experimentation deform *irreversibly* above a certain level of applied stress (i.e., they are really elastoplastic, not ideally elastic as the relevant theory supposes them). Likewise, some liquid crystals respond to stresses differently when magnetic fields (which count as trans-mechanical agencies) are present.

Thus an inflection point occurred *circa* 1750 as the science of motion exited its adolescence. Specifically, we see a deep shift in the form and status of laws of motion. The shift is where early modern mechanics turns into classical mechanics as we know it. Philosophically speaking, the inflection was genuinely deep. It changed the epistemic basis of mechanics; it altered its ontology in ways that we still struggle to grasp; and it had ripple effects on current ideas of what a law of nature is. In an important sense, the Long Scientific Revolution is still with us.[38]

[38] I am indebted to Katherine Brading for long, fruitful, and deeply illuminating conversations on the themes in my chapter. For helpful suggestions and constructive criticism, I am grateful to David Marshall Miller, Dana Jalobeanu, Zvi Biener, Brian Hepburn, Desmond Hogan, Adam Elga, Dan Garber, Elliott Chen, Adam Chin, and Jeremy Heis. For stimulating discussions, I thank audiences at Iowa State, Princeton, UC Irvine, and Caltech.

22

The Debate About Body and Extension

GEOFFREY GORHAM AND EDWARD SLOWIK

There were many debates about body in the Scientific Revolution. Points of contention included whether body is infinite in division or extent; whether body is active; the relation between body and mind; and how bodies are differentiated. While we will touch on many of these issues below, our focus will be on the debate about the relation between body and extension, for this was a direct consequence of the widespread rejection of the Scholastic conception of body as a composite of matter and form (hylomorphism). The other debates, meanwhile, derived from the diverse, often puzzling theories of the body–extension relation developed by the competing early modern systems of science and metaphysics that aimed to supplant Scholasticism.

The Scholastics developed Aristotle's basic idea that matter provides the *subject* of a body, while a substantial form makes the material subject this or that *kind* of substance, and thereby explains its features and powers.[1] A bit of matter is flammable and buoyant, for example, because it possesses the substantial form of wood rather than stone.[2] However, this notion of substantial form was commonly dismissed as useless or unintelligible by moderns like René Descartes: "no natural action at all can be explained by these substantial forms since their defenders admit that they are occult and that they do not understand them themselves."[3] But matter deprived of form seems to have no essence or qualities of its own, as Aristotle himself observed,[4] and this seems as unintelligible or useless as substantial form – body cannot be *mere* substratum. The obvious solution is to attribute to

[1] *Physics* II.1 193b1–11 (Aristotle 1984, 1:330).
[2] Although not an Aristotelian notion, the notion of "substantial form" became extremely important among the late Scholastics. See Des Chene (1996); Ariew (2011).
[3] Descartes (1964–1974, 3:506); Descartes (1984–1991, 3:208). See also Locke (1975, III.vi.10).
[4] *Metaphysics* VII.3 1029a20–26 (Aristotle 1984, 2:1625).

matter *at least* geometric quantity in three dimensions – i.e., extension – since this seems both essential to body and intelligible in itself.[5]

We can now appreciate the urgency of the debate that concerns us in this chapter: is extension *all there is* to body, or does body require some additional determination? We can also see how heavily this bears on the debates mentioned above. If body is simply extension then: (i) body is infinitely divisible and infinitely extended, since geometric quantity is continuous and there is no boundary to mere extension; (ii) body is inactive, since mere extension does not act; (iii) body is distinct from mind, since mere extension and thought have nothing in common; and (iv) body is individuated by something other than extension, since no part of three-dimensional extension is intrinsically distinct from any other. As we will see below, there is a rich diversity of views among early modern natural philosophers about how extension relates to body, and what this implies for doctrines (i)–(iv), among others.

To help frame and compare these diverse views of the extension–body relation, we find it useful to rely on the taxonomy of distinctions employed by Descartes, partly because it derives from earlier theories of distinction,[6] and partly because it was routinely employed by the early modern actors themselves. In his mature discussion of distinctions, Descartes maintains the following tripartite division:

(i) *A* and *B* are merely *conceptually distinct* when we cannot clearly and distinctly conceive a distinction between a substance and its attribute or between two attributes. Examples are the distinctions between a substance and its duration, and a thing's extension and its divisibility.[7]

(ii) *A* and *B* are *modally distinct* when we can conceive a substance *A* apart from a mode *B*, but not conversely; or when *A* and *B* are modes of the same substance. For example, we can conceive a substance apart from its shape, or a thing's shape apart from its motion.[8]

[5] This line of thinking has a long history. Already in the Middle Ages philosophers formulated this kind of (Platonic and Neopythagorean) criticism of the mainstream Aristotelian position; in the late sixteenth century and the beginning of the seventeenth century, a significant number of "Aristotelian" philosophers attributed extension to matter or equated matter with some form of Platonic substratum (having at least the property of extension). See Des Chene (1996, ch. 4); McCracken (1998a); Leijenhorst, et al. (2002, 388–389); Jalobeanu (2013, 215–216).

[6] Suárez (1976); Ariew (2011, 50–55).

[7] Descartes (1964–1974, 8A:30–32); Descartes (1984–1991, 1:214–215).

[8] Descartes (1964–1974, 8A:29–30); Descartes (1984–1991, 1:213–214).

(iii) *A* and *B* are *really distinct* when we can clearly and distinctly understand substance *A* apart from substance *B*, and conversely.[9]

In the following, we chart how the most prominent modern philosophers understood body and extension to be distinct (or not) according to this taxonomy.

Descartes

In general, body and extension are merely conceptually, and not really, distinct for Descartes. He equates the existence of body with that of pure extension in length, breadth, and depth. Strictly speaking, then, body possesses only the "primary" properties of extension – size, shape, motion, and the arrangement of parts – while the "secondary" properties, such as color and taste, are derived from them. This departs from Scholastic hylomorphism, wherein *all* (essential) properties are imbued by substantial form. Overall, Descartes's strategy is similar to the methodology of the ancient atomists, as well as of rival seventeenth-century corpuscular natural philosophies, such as Pierre Gassendi, in that he attempts to account for the variety of natural phenomena relying solely on the impact and aggregation of merely extended material particles. Nevertheless, Descartes adheres to some of the most important tenets of seventeenth-century Aristotelian natural philosophy. In particular, he insists that matter is infinitely divisible and he denies the existence of void space. While Descartes's corpuscles function much like atoms in the explanation of macroscopic phenomena, he rejects any form of atomism that posits a smallest indivisible material particle. Rather, since any given spatially extended length is conceptually divisible, God's omnipotence ensures that any extended body can be actually divided.[10] Likewise, his equation of spatial extension with body places him much closer to Scholasticism than Gassendi, who accepted void spaces completely empty of matter.[11]

In *Principles of Philosophy* (1644), his chief work on natural philosophy, Descartes posits extension as the "essence" of material substance. Extension constitutes both body and the space it inhabits: "the extension in length, breadth, and depth which constitutes the space occupied by a body, is exactly the same as that which constitutes the body."[12] Accordingly, a body's space – its "internal place" – is a mere conceptual abstraction from the body itself:

[9] Descartes (1964–1974, 8A:28); Descartes (1984–1991, 1:213).
[10] Descartes (1984–1991, 1:231–232). [11] Dobre (2011). [12] Descartes (1982, 43).

"Nor in fact does space, or internal place, differ from the corporeal substance contained in it, except in the way in which we are accustomed to conceive of them."[13] Individual bodies, meanwhile, are differentiated by their "external place," the two-dimensional surface or "boundary between the surrounded and surrounding body."[14]

This internal/external place dichotomy, like many other aspects of Descartes's natural philosophy, is a Scholastic concept,[15] as is the nominalism evident in his overall approach to space and place. That is, Descartes conceives the universal or abstract properties associated with bodies, which include space or place, as mere names that can be interpreted as (or reduced to) individuals: "For in fact the names 'place' and 'space' do not signify a thing different from the body which is said to be in the place: but only designate its size, shape, and situation among other bodies."[16] Conversely, we speak of space itself distinct from body (as when we say a body moves *through* space) only by considering the "generic unity" of bodily extensions:

> we attribute a generic unity to the extension of the space [of a body], so that when the body which fills the space has been changed, the extension of the space itself is not considered to have been changed or transported but to remain one and the same . . . .[17]

Several features of Descartes's broader natural philosophy and cosmology follow from his metaphysical conception of body, especially his insistence on a plenum (i.e., matter-filled) world and rejection of void space. He argues, "That a vacuum in the philosophical sense of the term (that is, a space in which there is absolutely no substance) cannot exist is evident from the fact that the extension of space . . . does not differ from the extension of body."[18] Not even a divine cause could bring about a vacuum, since, if God removed the matter from a vessel, no extension and thus no distance would remain, either, and "the sides of the vessel would thereby become contiguous to each other."[19] Here, the traditional Aristotelian substance–property metaphysics also plays a role, for Descartes adds that extension alone would constitute an accident or attribute without an underlying substance, which is impossible: "From the sole fact that a body is extended in length, breadth, and depth; we rightly conclude that it is a substance: because it is entirely contradictory for that which is nothing to possess extension."[20]

[13] Descartes (1982, 44). [14] Descartes (1982, 46). [15] Garber (1992, 148–155).
[16] Descartes (1982, 45). [17] Descartes (1982, 44). [18] Descartes (1982, 46–47).
[19] Descartes (1964–1974, 2:482, 4:109); Descartes (1982, 48). [20] Descartes (1982, 47).

This line of reasoning likewise rules out the Scholastics' notion of a void space outside of the finite material world; i.e., the "imaginary space" hypothesis of a non-extended void that can receive extended bodies.[21] Descartes reasons "that this world, that is, the whole universe of corporeal substance, has no limits to its extension," since

> no matter where we imagine the boundaries to be, there are always some indefinitely extended spaces beyond them, which we not only imagine but also perceive to be imaginable in a true fashion, that is, real. And it follows that these spaces contain corporeal substance which is indefinitely extended.[22]

Against the Scholastics, Descartes concludes that there is no empty space beyond our finite world, and that it must be "indefinitely" extended.

Numerous complications bedevil Descartes's attempt to equate body with spatial extension, especially in the context of his account of motion. In the *Principles*, for instance, "motion" is defined as

> *the transfer of one piece of matter or of one body, from the neighborhood of those bodies immediately contiguous to it and considered at rest, into the neighborhood of others*. By *one body*, or *one part of matter*, I here understand everything which is simultaneously transported.[23]

There is an apparent circularity in the definitions of motion and body – i.e., after defining motion as a transfer of a body from the neighborhood of its contiguous bodies, he proceeds to define body as that which is simultaneously transported. It seems to follow that a body at rest would merge into the surrounding plenum, for if body is "everything which is simultaneously transported," then a resting body could not be differentiated from the surrounding plenum matter that forms its contiguous neighborhood.[24]

Another concern involves the very possibility of motion in a plenum. Given the fact that all places are already filled with body, there is no place for a body to move into. Descartes strives to resolve this problem by claiming that all motion must ultimately be circular. He argues, "It has been shown . . . that all places are full of bodies . . . . From this it follows that no body can move except in a complete circle of matter or ring of bodies which all move at

[21] Grant (1981). [22] Descartes (1964–1974, 8A:52); Descartes (1984–1991, 1:232).
[23] Descartes (1982, 51).
[24] A fact that did not escape Descartes's followers and critics in the seventeenth century. Perhaps the most notorious criticisms of these points can be found in Isaac Newton's *De gravitatione* and Gottfried Wilhelm Leibniz's *De ipsa natura*. See Jalobeanu (2007); Brading (2011); Dobre (2011); Janiak (2015, ch. 4); Domski (2019).

the same time."[25] But since the bodies that partake in the ring all move simultaneously, his definition of body would entail that there is only one ring-shaped moving body, not many.

Descartes's conceptions of matter and motion also raise difficulties for his various hypotheses involving mechanical impact, such as his Laws of Nature. Descartes rejects impenetrability as a primary property of body, and attempts to derive impenetrability from spatial extension by insisting that "no two [bodily extensions] can in any way be conceived as penetrating each other at the same time in one and the same place, since it is contradictory for this to happen without some parts of space being removed."[26] In other words, the impossibility of two overlapping spatial extensions is supposed to refute the possibility of two overlapping bodies. But if, as Descartes claims, the solidity of a body is entirely due to the relative rest of its constituent material parts, and not to any material bond between those parts,[27] then any impact on the body should result in disparate motion of its parts, and thus its dispersion or destruction.[28]

Descartes's emphasis on extension as the sole essence of body would also seem to imply the passivity of body. Yet Descartes allots a central role to the activity of bodily forces or tendencies in his natural philosophy, such as the tendency of bodies to follow straight lines, or the resistance of a large resting body to the motion of a smaller moving body. In his early *The World*, he writes that "the virtue or power in a body to move itself can well pass wholly or partially to another body and thus no longer be in the first; but it cannot no longer exist in the world."[29] This explanation apparently envisions force as being akin to a property possessed by individual material objects, much like the qualitative, formal properties of the Scholastics. And while the later *Principles* often strives to repudiate these types of descriptions – e.g., motion "is a *transference*, not the force or action which transfers"[30] – the reliance on force-based descriptions of bodily phenomena is not entirely eliminated.

The insurmountable obstacles raised by Descartes's attempt to employ spatial extension alone in order to account for material phenomena prompted many subsequent natural philosophers to assign additional basic properties to body beyond those of extension, such as impenetrability or

[25] Descartes (1982, 55–56). [26] Descartes (1964–1974, 5:271); Descartes (1984–1991, 3:362).
[27] Descartes (1982, 70).
[28] For a detailed discussion of the difficulties involved in Descartes's notion of motion and collisions, see Des Chene (1996, chs. 8–9); Slowik (2002, chs. 3–4). See also Murray, et al. (2011).
[29] Descartes (1964–1974, 11:15). [30] Descartes (1982, 51).

cohesion, as we shall see. These same difficulties have also led modern scholars to divergent interpretations of Descartes's view of the relationship between force and matter. On one interpretation, forces really exist in bodies as properties or modes,[31] whereas another considers force as a sort of shorthand description of the dynamical regularities maintained in the world by God, and not as quality inherent to bodies.[32]

Gassendi

Among seventeenth-century philosophers, Gassendi was the first to defend thoroughly the classical atomist conception of space as an infinite, three-dimensional extension independent of body.[33] In *Syntagma philosophicum* (1658), his major work on natural philosophy, he identifies a number of attributes of space. According to Gassendi, space is:

(i) *infinite* – space has no limit in magnitude or duration;
(ii) *independent* – space is independent of body;
(iii) *dimensional* – space has length, width, and depth, equivalent to body;
(iv) *unified* – there is one space at all times;
(v) *homogeneous* – space is the same everywhere;
(vi) *indivisible* – space has no gaps;
(vii) *related to creation* – space surrounds the finite extensions of things and the world; and
(viii) *related to the creator* – God is everywhere in space (immense).[34]

Attributes (i)–(vi) provide the structure of absolute space (and time) later championed by Isaac Newton. What is noteworthy here is the radical implication of a *real distinction* between space and body, and the concomitant possibility of space void of body.

Gassendi's defense of void space relies on a mix of traditional thought experiments and empirical observations. His main argument is an elaborate thought experiment involving the annihilation of matter. This supposes that God first evacuates the entire lunar sphere while leaving the sphere itself intact; then successively creates and destroys an endless sequence of increasingly expansive worlds; and finally restores the original world by an act identical to the original creation. There are two crucial moves in

[31] Gabbey (1980). For a discussion of the debate see also Hattab (2000).
[32] Garber (1992). See also Domski (2020).
[33] LoLordo (2006); Bellis (2018).
[34] Gassendi (1658b, I.2.i.7, 224–225); Gassendi (1972, 395–396); Čapek (1976, 199–201). See also Charleton (1966, 75–78).

Gassendi's argument. First, he invokes the intuition that the void within the lunar sphere retains the determinate dimension, shape, and quantity fixed by the inner surface of the sphere:

> I ask whether in this empty region while this spherical lunar heaven still persists we do not conceive a distance or interval between one point of its concave surface and another opposite to it? Has not this distance some length, that is an incorporeal and indivisible line, which would be the diameter of that region?[35]

In other words, if God were to evacuate a vessel – here, the lunar sphere – of all matter, a distance between its sides would remain, *contra* Descartes. Yet if God were to subsequently annihilate the rest of the world, the same real dimensions and distances would remain in the superlunary realm, as well: "in both regions there would equally exist as many spatial dimensions as had existed corporeal dimensions in the whole world."[36] Thus, Gassendi uses this "partial annihilation" argument as a bridge to transfer real dimensions and distances from the material plenum to the void. And, as John Locke and Newton would later do, Gassendi rejects the Aristotelian assumption that space, so conceived, must be either substance or accident.[37]

Gassendi has very little to say about the nature of bodies, besides conceiving of them as constituted by matter, which is to say by atoms. In general, Gassendi hews closely to the empiricist attitude that bodily extension is obviously distinct from space, while resisting speculation about the essence of body itself: "knowledge of the existence of a thing has no necessary connection with knowledge of its essence or inner nature."[38] Consequently, he does not explain how a body's extension is differentiated from the space it inhabits. He simply insists, against Descartes, that extension is a mode of matter rather than its essence.

Hobbes

Since "in teaching philosophy the first beginning is from definition,"[39] we should begin with Thomas Hobbes's definitions of body. In *Leviathan* (1651), body is "that which filleth some certain room or imagined place, and dependeth not on the imagination but is a real part of what we call the

[35] Gassendi (1658b, I.2.i.1, 224–225); Gassendi (1972, 387); Čapek (1976, 92). See also Charleton (1966, 67).
[36] Gassendi (1658b, I.2.i.1, 224–225); Gassendi (1972, 387); Čapek (1976, 92).
[37] LoLordo (2006, ch. 5). [38] Gassendi (1972, 184).
[39] *De corpore* I.6.15 (Hobbes 1839–1845, 1:85).

universe."[40] Similarly, in *De corpore* (1655), "a body is that, which having no dependence upon our thought, is coincident or coextended with some part of space."[41] Unlike the bodies that fill it, space itself is strictly imaginary and mind-dependent for Hobbes. It is "the phantasm of a thing existing simply insofar as it exists."[42] Yet Hobbes admits that, insofar as it is filled by concrete body, space is perfectly "real": "the extension of a body, is the same with the magnitude of it." This "real space," Hobbes insists, "does not depend on our thought, as imaginary space does."[43]

Thus far, Hobbes agrees with Descartes that anything real and extended is body. However, Hobbes seems to regard this as a contingent fact about what we know of body, rather than a conceptual fact about the nature of extension itself.[44] Thus, in *De corpore*, after distinguishing body from space, Hobbes notes that "certain philosophers" conclude that the corporeal world is infinite because "taking space to be the extension of bodies and thinking extension may increase continually, [they] infer that bodies may be infinitely extended."[45] Hobbes probably has in mind Descartes's argument in the *Principles of Philosophy*, mentioned above, that wherever we imagine boundaries to extension we are forced to recognize that there must be indefinitely extended space beyond them. Hobbes's objection is not to the "continual increase" of extension, but to the Cartesian strict identification of extension and body. He believes it is empirically, not conceptually true that the world is a plenum.[46] While acknowledging that "no body can be conceived without extension," he also insists that when we imagine "that a body fills space or is coextended with it," we recognize "that coextension is not the coextended body."[47] He also says two bodies are not contiguous if "there intercede any imagined space which may receive another body," and ridicules those men who "being otherwise skillful in philosophy" (presumably Descartes – recall the divinely evacuated vessel discussed above) maintain that "two bodies must therefore necessarily touch one another because no other body is between them."[48]

[40] *Leviathan* III.34.2 (Hobbes 1839–1845, 3:381; Hobbes 1994b, 261).
[41] *De corpore* II.8.1 (Hobbes 1839–1845, 1:102; Hobbes 1845, 1:91).
[42] *De corpore* II.7.2 (Hobbes 1839–1845, 1:94; Hobbes 1845, 1:83).
[43] *De corpore* II.8.4 (Hobbes 1839–1845, 1:105; Hobbes 1845, 1:93).
[44] See, for example, Adams (2016); Biener (2016); Jesseph (2020).
[45] *De corpore* II.7.2 (Hobbes 1839–1845, 1:93; Hobbes 1845, 1:83).
[46] *De corpore* IV.26.2 (Hobbes 1839–1845, 1:414–415; Hobbes 1845, 1:338).
[47] *De corpore* II.8.2–3 (Hobbes 1839–1845, 1:102–104).
[48] *De corpore* II.8.9 (Hobbes 1839–1845, 1:108–109).

As another plenist, Hobbes, like Descartes, faces the problem of body differentiation: What distinguishes one part of the plenum from another, and what makes a given part the same body over time? Hobbes has little to say about this. In the *De corpore* chapter "Of Identity and Difference," Hobbes says "it is obvious that no two bodies are the same, as seeing as they are two they are in two places at the same time."[49] This is hardly a solution, of course, since the question concerns how to pick out distinct bodies within the plenum to begin with. Hobbes might have regarded differentiation as subjective or purely nominal. This is suggested by his remark that division among the parts of space and time "is not made by the operation of the hands but of the mind."[50]

Late in his career, Hobbes drew an interesting distinction between 'body' *simpliciter* and 'a body': "*Body* (Latin: *corpus*; Greek: σώμα) is that substance which hath magnitude indeterminate and is the same with corporeal substance; *A body* is that which hath magnitude determinate and so is understood to be a *totum* or *integrum aliquid*."[51] Unlike "a body," which is a "complete or entire something," and which satisfies the *Leviathan* definition above, mere "body" has no determinate size and so need be neither finite nor imaginable.[52] The distinction occurs in the context of Hobbes's account of his "corporeal deity" – "a most pure, simple, invisible spirit corporeal,"[53] so it seems, in the end, that Hobbes wants to make room for body that is infinite and, as such, unimaginable.

Thus, for Hobbes, extension in a narrow sense – i.e., corporeal magnitude – is at most conceptually distinct from body. But in a broad sense, which would include consideration of imaginary space, body and extension are not merely conceptually distinct, but *really* distinct, because the distinction between the real and the imaginary is itself real.

Spinoza

In his earliest work, Baruch Spinoza parrots Descartes: "where there is extension or space there, of necessity, substance also exists."[54] Later, in the *Ethics* (1677), Spinoza defines 'body' as a "certain and determinate" expression of God considered as an extended thing.[55] This amounts to saying that bodies

[49] *De corpore* II.11.2 (Hobbes 1839–1845, 1:133).
[50] *De corpore* II.7.5 (Hobbes 1839–1845, 1:96).
[51] Hobbes (1839–1845, 4:309).
[52] Hobbes (1839–1845, 4:393).
[53] Hobbes (1839–1845, 4:313).
[54] *Principia philosophiae cartesianae* II, Lemma 1 (Spinoza 1998, 49).
[55] *Ethics* II.P1.def. Here and below, quotations are from Spinoza (1985), cited by book and proposition.

are finite modes. "Individual things are nothing but modifications of the attributes of God, or modes by which the attributes of God are expressed in a fixed and definite manner."[56]

Although body expresses the attribute of extension, Spinoza insists the two not be conflated. God is certainly an extended thing,[57] but God is not divisible in the way bodies are. Spinoza explains that the infinite extension of God is "not measurable" and "not composed of finite parts" such as bodies.[58] Notably, Spinoza's opposition to the real division of extension seems to reflect his ambivalence about the new science in general, and about the mathematization of nature in particular. Thus, in the "Letter on the Infinite," he bemoans that "they talk utter nonsense, not to say madness, who hold that extended substance is put together of parts really distinct from one another."[59] In any case, since the extension of God allows no real division, the parts of it "are distinguished in it only insofar as we conceive matter to be affected in different ways, so that its parts are distinguished only modally, but not really."[60] He thus draws a conceptual distinction between quantity as (superficially) conceived by the imagination, which is "finite, divisible, and composed of parts" and (abstractly) conceived by the intellect, which is "infinite, unique, and indivisible."[61] The merely imaginary division of infinite extension into finite "parts" also seems consistent with Spinoza's basically Cartesian account of bodies in the "physical digression" in Part II of the *Ethics*, where he writes that "bodies are distinguished from one another by reason of motion and rest, speed and slowness, and not by reason of substance."[62]

Despite this rather obscure conception of extension and its relation to finite bodies or modes, Spinoza follows Descartes in his mature writing by identifying corporeal extension with space. Aside from the arguments just mentioned, this is also clear from his brief discussions of the vacuum. Spinoza argues that if corporeal substance could be really distinguished into parts then that would allow a vacuum. But since, as he claims to have established elsewhere, "there is no vacuum in nature," neither can extension be really divided into parts.[63] The "elsewhere" that a vacuum is confuted seems to be the early *Principia philosophiae cartesianae* (1663), cited above, where Spinoza

[56] *Ethics* I.P25.corollary. [57] *Ethics* I.P14.corollary 2. [58] *Ethics* I.P15.scholium.v.
[59] Spinoza (2016, 202). For an incisive recent discussion of Spinoza on the infinite, see Melamed (2013).
[60] *Ethics* I.P15.scholium.v. [61] *Ethics* I.P15.scholium.v. [62] *Ethics* II.lemma 1.
[63] *Ethics* I.P15.scholium.iv.

argues that extension "must be attributed to something" – i.e., to a substance. A vacuum would be a "pure nothing," *per impossibile*.[64]

So, like Descartes, the early and apparently the later Spinoza excludes *a priori* empty extension. Nevertheless, finite bodies for him are modally, if not really, distinct from extension.

Locke

Whereas Locke's view of the body–extension relationship was originally close to Descartes's, he gradually departed from it while revising the *Essay concerning Human Understanding*. In the earliest known draft (1671), Locke closely associates extension with body, which he remarks is "the thing most properly capable of extension."[65] But in a more detailed draft from the same year, he renounces the reduction of the idea of extension to that of body: "we can apply in our thoughts the ideas of these lengths to those distances beyond the confines of the world where there are no bodies at all."[66] He still insists, however, that extension is merely conceptually (rather than really) distinct from body, "which is the only real thing that exists and has extension that we know."[67] The continued drift away from Cartesianism is evident in notebooks from a few years later, where he introduces the abstract notion of distance as "a simple relation arising [between] two bodies or beings that do not immediately touch one another,"[68] not as the mode of any individual body. But Locke remains emphatic that, despite the conceptual distinction between distance or extension and body, "extension separated in our thoughts from body seems to have no more real existence than number has without anything to be numbered."[69]

In the third and penultimate *Essay* draft (1685), Locke links himself with the Scholastic imaginary space tradition, conceiving of space or extension beyond the world as the bare capacity for receiving corporeal dimensions. He writes, "the idea of Extension of any body is so much of that imaginary space as the bulk of that body takes up or lies betwixt the extremities of it."[70] But in the published *Essay* itself (1690), he is derisive of the qualifier 'imaginary': "they term what is beyond the limits of the universe imaginary space: as if it were nothing because there is no body existing in it."[71] And Locke insists, just as

[64] For more detailed discussion of Spinoza on extension, body, and space, see Schmaltz (1999); Peterman (2015).
[65] Locke (1990, 79). [66] Locke (1990, 243). [67] Locke (1990, 253). [68] Locke (1935, 77).
[69] Locke (1935, 77). [70] "Draft C" xviii.11. See Locke (2021).
[71] *Essay* II.xv.4 (Locke 1975, 198). See Ayers (1991, 1:235).

strongly as Newton, on the real distinction between "those uniform Boundless Oceans of Duration and Space" and "certain fixed points in sensible beings we reckon, and from them we measure out portions of those infinite quantities."[72]

It is likely Locke's ultimate real distinction of extension from body was influenced by his close reading (and review) of Newton's "never enough to be admired book,"[73] *Philosophiae naturalis principia mathematica* (1687), shortly after its publication. But more obvious in the *Essay* is his opposition to Descartes, who "would persuade us that body and extension are the same thing."[74] In response to Descartes's famous demonstration, noted above, that "if nothing be between bodies they must necessarily touch,"[75] Locke answers that mere extension or space *is* a "something" that transcends both the Cartesian dichotomy of body and spirit and the Aristotelian dichotomy of substance and accident.[76] Consequently, empty space can exist between separated bodies.

Another target in Locke's treatment of body seems to be Hobbes. Against those who "conclude the essence of body to be extension because, they say, they cannot imagine any sensible quality of any body without extension," Locke points out (i) ideas of taste and smell do not include extension, and (ii) even if extension were included in all ideas, that would not make extension their essence – otherwise unity would be the essence of all things.[77]

By different routes, the rationalist Descartes and empiricist Hobbes were led to the same inadequate conception of body as extension alone, which fails, as we have noted, to respond to the problem of differentiation. Locke's own response is to recognize that our idea of body includes, besides extension, solidity. This property distinguishes individual bodies from the space they inhabit, for "by this idea of solidity is the extension of body distinguished from the extension of space."[78] Locke says that if someone asks what the idea of solidity is, "I send him to his senses. Let him put a football or a flint between his hands and then endeavour to join them and he will know."[79] Indeed, solidity (or impenetrability) is the idea most commonly derived in sense. We derive it from the physical resistance we always encounter with gross bodies – "we always feel something under us, that supports us, and

[72] *Essay* II.xv.5 (Locke 1975, 198).
[73] *Essay* IV.vii.3 (Locke 1975, 599).
[74] *Essay* II.xiii.11 (Locke 1975, 171).
[75] *Essay* II.xiii.16 (Locke 1975, 173).
[76] *Essay* II.xiii.16–17 (Locke 1975, 173–174).
[77] *Essay* III.xii.24 (Locke 1975, 178). For a recent "steadfast" relationist reading of Locke on space, see Thomas (2016). For an absolutist reading of the mature Locke, see Gorham and Slowik (2014); Gorham (2020). A more ambivalent view is found in Jacovides (2016).
[78] *Essay* II.vi.5 (Locke 1975, 126).
[79] *Essay* II.iv.6 (Locke 1975, 126–127).

hinders us further sinking downwards" – and we generalize this resistance to all bodies and suppose it "inseparably inherent in body, wherever or however modified."[80] For the Locke of the *Essay*, then, solidity is the essential property of body that differentiates it from extension.

In his 1729 translation of the *Essay*, Pierre Coste attributed Locke's "dim and seeming" speculation about "how matter might at first be made"[81] to the influence of Newton's theory of body from *De gravitatione* (see below), based on Newton's personal report of a conversation with Locke. On this theory, as Coste puts it, body is wherever "God through his power had prevented anything from being able to enter a certain portion of pure space."[82]

But even if Newton was not his inspiration, Locke offers four interesting observations about solidity. First, it is by solidity that we conceive a body "filling" pure space.[83] Second, solidity is not a matter of degree. The softness of an object is due to weak cohesion among, not to any deficient solidity of, its individual parts. Thus, "all the bodies in the world pressing a drop of water on all sides will never be able to overcome the resistance which it will make, soft as it is."[84] Third, the resistance, protrusion, and impulse of bodies all depend on their solidity.[85] Notice that this accounts for the activity of bodies Descartes could not explain. Fourth, just as the ideas of body and of color depend on extension, without being the same as extension, so, although "'tis true solidity cannot exist without extension," it is a distinct idea.[86] There is much to query about these observations. But, to summarize, the mature Locke certainly thinks that extension can (and perhaps does) exist really distinct from bodies; but not vice versa.

Leibniz

In sharp contrast with Descartes's claim that extension is the essence of corporeal substance, Gottfried Wilhelm Leibniz's late metaphysics regards extension and other spatial properties as phenomenal, derivative features of his simple substances, or "monads." Leibniz contends that

> the notions of size, shape, and motion are not as distinct as imagined and that they contain something imaginary and relative to our perception, as do (though to a greater extent) color, heat, and other similar qualities, qualities

[80] *Essay* II.iv.1 (Locke 1975, 123).
[81] *Essay* IV.x.18 (Locke 1975, 628).
[82] Bennett and Remnant (1978, 5). See also Downing (2014).
[83] *Essay* II.iv.2 (Locke 1975, 123).
[84] *Essay* II.iv.3 (Locke 1975, 124).
[85] *Essay* II.iv.5 (Locke 1975, 126).
[86] *Essay* II.xii.11 (Locke 1975, 172).

> about which one can doubt whether they are truly found in the nature of things outside ourselves.[87]

In short, Leibniz does not accept a clear and determinate primary/secondary distinction among the properties of bodies, since he places the so-called primary properties, such as shape, size, and motion, alongside the secondary properties, like color and heat. All of these are dependent on perception, and not necessarily real. Monads, on the other hand, are real, but are non-extended, without parts, and form aggregates, some of which are bodies ("bodies are only aggregates"[88]). While bodies possess an ideal unity,[89] Leibniz also insists that "properly speaking, matter is not composed of constitutive unities [i.e., monads], but results from them."[90] Therefore, in the Cartesian taxonomy introduced at the outset, Leibniz's body and extension are *modally distinct*, since they are both dependent, modal features of the simple substances, the monads.

If bodily extension is downgraded in Leibniz's mature metaphysics, the concept of force assumes the central role.

> We must realize, above all, that force is something absolutely real in substances, even in created substances, while space, time, and motion are, to a certain extent, beings of reason, and are true or real, not *per se*, but only to the extent that they involve either the divine attributes (immensity, eternity, and the ability to carry out works), or the force in created substances [i.e., monads].[91]

Leibniz offers a complex distinction between primitive force, which is associated with a monad's essence or substantial form, and derivative force, which functions as a modification or limitation of primitive force.[92] This distinction is also implicated in his account of body: "when force is taken for the principle of action and passion, and is therefore something modified through derivative forces, . . . you can understand well enough from what has been said that this principle is bound up with the very notion of [bodily] extension."[93] Primitive forces, that is, provide for the activity (and passivity) of extended bodies.

[87] Leibniz (1965, 4:437); Leibniz (1989b, 44). There is a rich literature on Leibniz's criticism of Descartes and his alternative solution to the Cartesian problem of what has been called the "dissolution of bodies" (Jalobeanu 2013). A relevant selection would include Adams (1983); Phemister (1999); Crockett (2005); Garber (2009); Garber (2011); Levey (2011).

[88] Leibniz (1965, 7:344); Leibniz (1989b, 319).

[89] Leibniz (1965, 2:256).

[90] Leibniz (1965, 2:268); Leibniz (1989b, 179).

[91] Leibniz (1962b, 6:247); Leibniz 1989b, 130).

[92] Leibniz (1965, 2:251, 4:512–513); Leibniz (1989b, 162–163, 176).

[93] Leibniz (1965, 2:268–270); Leibniz (1989b, 179–180).

Yet, even if his force-based conception of body marks a novel contribution to early modern theories of matter, Leibniz nonetheless remained committed to the mechanical philosophy and its reliance on bodily extension and motion to explicate the phenomena of material bodies. He insists that "everything happens mechanically in nature," and that "force is exercised only through an impressed impetus."[94] Newtonian gravity, which Leibniz describes as "incorporeal" and similar to "occult qualities," is thus rejected.[95]

Newton

In *De gravitatione*, a work that pre-dates the *Principia*, Newton develops a hypothesis – inspired by Henry More and Walter Charleton – about the nature of body that resembles Spinoza's monism. Briefly put, Newton posits a world where God instantiates the familiar bodily properties in space. Thus, spatial extension itself serves as the foundation of bodily properties, and an underlying corporeal substance is not required to house bodily accidents:

> If [God] should ... cause some space projecting above the earth, like a mountain or any other body, to be impervious to bodies and thus stop or reflect light and all impinging things, it seems impossible that we should not consider this space really to be a body from the evidence of our senses ....[96]

The imperviousness of a region of space is sufficient for corporeality.[97] Thus, Newton stipulates that "we can define bodies as *determined quantities of extension which omnipresent God endows with certain conditions*."[98] Newton further asserts that these "conditions" are:

(i) mobile, insofar as they can successively endow different spatial regions;
(ii) capable of bringing about perceptions in minds; and
(iii) not susceptible to interpenetration, so that no spatial region can be endowed with multiple instances of a property.

Given such characteristics, Newton reasons that these "determined quantities of extension" can account for the phenomena of material bodies without the need for Descartes's material substance or the Scholastics' notion of prime matter.[99]

[94] Leibniz (1965, 7:337–344); Leibniz (1989b, 317–319). [95] Slowik (2020).
[96] Newton (2004, 27–28). [97] Slowik (2013). [98] Newton (2004, 28).
[99] Newton (2004, 27–31).

In the Cartesian taxonomy, this version of Newton's ontology exemplifies a *modal* distinction between body and space, since body is a property (or properties) of space. Yet, if the dependence of space on God in Newton's ontology is also acknowledged (he writes, "space is eternal in duration and immutable in nature because it is the emanative effect of an eternal and immutable being"[100]), then it would appear that spatial extension itself is a mode, as well. God's being provides the ontological ground for space; and space, in turn, provides the ontological ground for body.

Although Newton's "determined quantities of extension" theory had vanished from his work by the time of *Principia*, the "absolute true and mathematical space" of that text retains a robust structure independent of bodies, which have absolute place in virtue of occupying parts of absolute space.[101] While it may not be quite apt to say bodies are mere modes of extension in *Principia*, since they now seem to qualify as substance, space or extension certainly remains prior to body. Even God's existence requires space, since "certainly the maker and lord of all things will not be never or nowhere."[102]

Regarding the properties of body, Newton parts company with the mechanical philosophy in his willingness to admit "active principles" that cannot be reduced to the motions and impact of bodies. In the "Queries" appended to the *Opticks* (1721), he comments that his basic particles of matter "have not only a *vis inertiae*, accompanied with such passive laws of motion as naturally result from that force, but also that they are moved by certain active principles, such as is that of gravity, and that which causes fermentation, and the cohesion of bodies."[103] However, in earlier correspondence with Richard Bentley (1692–1693), Newton seems to rebuff the idea that gravity is a bodily property that can act at a distance.

> That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else . . . is to me so great an absurdity. . . . Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers.[104]

Accordingly, if Newton continued to hold the view that the cause of gravity must be local, then there are several possible interpretations, among them:

[100] Newton (2004, 26). [101] Newton (1999, 409). [102] Newton (1999, 941).
[103] Newton (2004, 137). [104] Newton (2004, 102–103).

(i) gravity is caused by a space-filling aether, a material cause that would uphold the view of the mechanical school that all bodily interactions involve bodily contact; or
(ii) gravity stems from a non-mechanical source, either as an immaterial being that directly acts on bodies (such as God), or an unknown non-mechanical property that God has superadded to matter (such as fermentation and particle cohesion).[105]

Newton, along with most natural philosophers of his time, had grave reservations about admitting non-mechanical material properties in their explanations, as in (ii). Nevertheless, those qualms had largely been discarded by the mid-eighteenth century, even if the exact nature of the non-mechanical force, and its relation to body, remained a mystery.

Conclusion

Early modern natural philosophers rejected the Aristotelian hylomorphic account of material bodies, particularly insofar as it simply attributed all the properties of a body to its substantial form, which was simultaneously explanatorily vacuous and mysteriously recondite. However, Descartes's response, the identification of body and geometrical extension, swung to the opposite extreme. His ontology was too impoverished to explain adequately bodily phenomena, or even the differentiation of individual bodies. The resulting contretemps generated a variety of positions that widened the Cartesian conceptual distinction between body and extension into a modal or even a real distinction. This, in turn, made room for force to replace extension as the central concept in the understanding of body, to the point of becoming – in the work of Leibniz – a novel version of the substantial form. These metaphysical developments, moreover, were intimately related to the advances in early modern physics described elsewhere in this volume. Consequently, they form the foundation of our modern understanding of the corporeal world.

[105] These and several other options have been subject to intense debates among Newton's followers and they remain, to date, topics of contention among scholars. For a useful survey, see Henry (2011).

23

Space and Its Relationship to God

ANDREW JANIAK AND EMILY THOMAS

Introduction

During the Scientific Revolution, philosophers intensely debated how best to understand space. Many debates revolved around the account René Descartes advanced in his *Principles of Philosophy* (1644), and this chapter treats it as a focal point. Descartes argued for a return to the Aristotelian view that there is no difference in reality between space and matter, entailing that empty space – space empty of matter – is impossible. Over the next century, all kinds of philosophers attacked this position, and we take their rejections of Cartesian space as a starting point for exploring alternative views.

Many important discussions of space emerged during this period, and we cannot cover them all.[1] We have chosen to present a varied selection in chronological order, from Henry More, Samuel Clarke, Isaac Newton, Catharine Cockburn, and Gottfried Wilhelm Leibniz. These philosophers are united in rejecting Descartes's account of space, and the breadth of theories they produce should demonstrate the metaphysical richness of this era. Newton and Leibniz are particularly important because the clash between their theories of space laid the groundwork for twentieth- and ongoing twenty-first–century debates between substantivalism and relationism. From the mid-seventeenth century, we will see that debate grew especially heated because of its theological implications – for many philosophers, the nature of space is deeply tied to the nature of God. Although the rejection of Cartesian space led to diverse accounts, we claim that there is nonetheless a deep agreement among these various alternatives: all the accounts agree on the *features* of space. As we explain below, this

[1] Our focus on philosophers responding to Descartes excludes figures such as Giordano Bruno, Francis Bacon, Pierre Gassendi, and Walter Charleton. Broader discussions of space in this period include Burtt (1924); Koyré (1957); Grant (1981); Pasnau (2011); Reid (2012); Huggett and Hoefer (2018); Thomas (2018).

fundamental agreement effectively set the scene for Immanuel Kant's theory of space, advanced after the Scientific Revolution had concluded.

Cartesian Space and Its Controversies

Descartes sets out his substance–attribute–mode ontology in the *Principles*, as follows: "By substance we can understand nothing other than a thing which exists in such a way as to depend on no other thing for its existence."[2] Independent substances thus are contrasted with "modes" – dependent entities that affect or modify substances. Also, to each substance belongs one "principal attribute" that constitutes its nature or essence, and to which all its other properties are referred. Extension in length, breadth, and depth constitutes the principal attribute of corporeal or material substance; and thought constitutes the principal attribute of incorporeal or thinking substance. As God is immutable, there are no modes in God, merely attributes.

For Descartes, things can be distinguished in different ways, as follows.[3] A "real distinction" holds between substances: we know two substances are really distinct if we can "clearly and distinctly" understand one apart from the other. Meanwhile, a "conceptual distinction" holds between a substance and some attribute of that substance without which it is unintelligible, or between two attributes of a single substance. A conceptual distinction is indicated by our "inability to form a clear and distinct idea of the substance if we exclude from it the attribute in question." To illustrate, Descartes argues there is a real distinction between a person's mind and body – these are two distinct substances, and we can form a clear and distinct understanding of one without the other. In contrast, there is only a conceptual distinction between a material substance and its attribute of extension, so in reality an extension must be considered as the extended substance itself. Similarly, there is only a conceptual distinction between God and God's attributes.

As there is only a conceptual distinction between material substance and its attribute of extension, Descartes then presents the following view of space:

> There is no real distinction between space . . . and the corporeal substance contained in it; the only difference lies in the way in which we are accustomed to conceive of them. For in reality the extension in length, breadth,

[2] Descartes (1964–1974, 8A:24–26); Descartes (1984–1991, 1:210–212).
[3] Descartes (1964–1974, 8A:28–31); Descartes (1984–1991, 1:213–215).

> and depth which constitutes a space is exactly the same as that which constitutes a body.[4]

In other words, there is no difference in reality between space and material body. A consequence of this view is that there cannot be space "empty" of matter. Where there is matter, there is space. Where there is space, there is matter. The Cartesian material world is a matter-stuffed plenum.

Descartes probably did not think his account of space and matter would be controversial, since it is essentially the same as Aristotle's.[5] Yet it proved controversial throughout the seventeenth and early eighteenth centuries, and still provokes disagreement today.[6]

Descartes's identification of space with matter was adopted in various ways by philosophers whose Cartesianism is well documented, including Jacques Rohault, Baruch Spinoza, and Nicolas Malebranche.[7] It can also be found far beyond that crowd. For example, in her *Grounds of Natural Philosophy*, the English philosopher Margaret Cavendish writes there cannot be any vacuum because "Body and Place is but one thing."[8] In his *Letters to Serena*, the Irish free-thinker John Toland agrees there is no space distinct from matter.[9] But, as we shall see, others rejected the view.

More: Space as God

The Cambridge Platonist Henry More (1614–1687) is perhaps best known for his views on space, which evolved over the course of his career. Descartes exerted a significant influence over More's early work.[10] For example, More's 1646 *Democritus Platonissans* adopts the Cartesian positions that the material world is a plenum, that there is no space "unstuffd with Atoms," and that material bodies move in circular vortices.[11] However, although More held

[4] Descartes (1964–1974, 8A:45); Descartes (1984–1991, 1:227).
[5] Aristotle also identifies space with matter; see his *Physics* IV.8 216b2–15 (Aristotle 1984, 1:367). Woolhouse (1994, 32) suggests Descartes's account of space deliberately alludes to Aristotle.
[6] Issues raised by Descartes's account include the following. How are bodies individuated – by motion, or some other way? On this, see Normore (2008, 280); Pasnau (2011, 153, 686). Are there many material substances in Descartes's universe, or should we see space as one giant material substance? On this, see Garber (1992, 181); Secada (2004, 242–243); Sowaal (2004). Finally, are material bodies phenomenal? On this issue, see Lennon (2007); Thomas (2015).
[7] On Rohault's theory of space, see Spink (2017); on Spinoza's extension see Shein (2018); on Malebranche's Cartesian metaphysics see Schmaltz (2017b).
[8] Cavendish (1668, 4).
[9] Toland (1704, 182–183).
[10] See Lamprecht (1935); Patrides (1969, 29–31); Gabbey (1982); Reid (2012, 23–26).
[11] More (1878, 90).

that the material world is a plenum throughout his career, he never adopted Descartes's *identification* of matter and space. In a letter dated May 5, 1651 to Anne Conway, More writes that, unlike Descartes, he holds that the notion of "empty space" – space devoid of matter – is not a contradiction. Indeed, More argues that there is infinite space beyond the finite material universe, and that it is possible for empty spaces to appear in the material universe. More explains that, although the material world is a plenum, it is finite, effectively positing an island of matter floating in an otherwise empty infinite space. In addition, More asks us to imagine a world containing just two "plaines" (i.e., flat slabs) of marble. If these slabs of marble lay on top of each other, and then moved apart, More claims a vacuum would come between them.[12]

More consistently rejected Descartes's account of space, but the details varied.[13] His mature account, for which he is best known, appears in his magnum opus, *Enchiridion metaphysicum*. Recall that, according to Descartes, space is unconnected with God, since they are distinct substances. On More's account, by contrast, space *is* God. Using thought experiments, More argues against Descartes that there is everywhere "a certain immobile extension diffused all round in all parts even to infinity and distinct from mobile matter."[14] More then goes on to use Descartes's substance–attribute–mode metaphysics in a decidedly un-Cartesian way, arguing that if we accept that extension is a real attribute of matter, we should also accept that extension is a real attribute of some other subject in the *absence* of matter:

> the real attribute of some real subject (*realis Subjecti Attributum reale*) can be found nowhere else except where in the same place there is some real subject under it. And, indeed, extension is the real attribute of a real subject (namely matter), which however is found elsewhere . . . . Therefore, it is necessary that some real subject be under this extension, since it is a real attribute.[15]

Extension is an attribute of matter, *and* extension is an attribute of some other "real subject" distinct from matter. What might that other real subject be?

More contends that this "immobile extension" or space *must* be divine as it shares at least twenty of God's titles, including being one, simple, immobile, eternal, complete, independent, and existing from itself.[16] Hence, space is an attribute of God. But More follows Descartes in holding there is no real distinction between a substance and its attributes, so More also holds there is

[12] Nicolson and Hutton (1992, 486–489). [13] See Reid (2012, 96–97).
[14] More (1995, I.vii.15, p. 1:52). [15] More (1995, I.viii.6, pp. 1:56–57).
[16] More (1995, I.viii.7–13, pp. 1:57–60).

no real distinction between God and space. This sometimes leads More to write that space is a substance – implicitly the substance of God. For example, immediately following his discussion of God's shared titles, he adds "that infinite extension which is supposed by the common people to be mere space, is indeed a certain substance, and that incorporeal, or spirit."[17]

Space is not God's only attribute. God is also omniscient, omnipotent, benevolent, and so on. More argues that we cannot clearly conceive an attribute without conceiving the substance to which it belongs.[18] As space is only one attribute of God, any attempt to consider space independently of God results, at best, in an unclear or confused *representation* of God. As More puts it, regarding the "internal space" occupied by an object,

> As when someone descries some object visibly, and, from a distance in the twilight, he apprehends it simply to be some opaque body, he does not know of what kind, animal, or tree, the backside of a horse ... so, indeed, is the matter here. The object of our mind which we say to be internal space is only a slight and diluted, and general, shadow (*umbra*) representing the nature of the uninterrupted divine presence under the obscure light of our intellect.[19]

More's understanding of the relationship between substances and attributes explains why he describes space as the *attribute* of God (conceptually we can distinguish between God the substance and God's attribute of immensity), as the *substance* of God (in reality, there is no distinction between God's substance and God's attributes), and also as a confused *representation* of God (when we try to consider space independently of God, we are not fully apprehending space or God).

Whereas Descartes identified space with the least perfect of substances, matter, More identified it with the most perfect, God. More's account of space would prove just as controversial as Descartes's, and it was attacked by a range of thinkers, including Margaret Cavendish and Leibniz. However, More's account also proved hugely influential, and was used as a source by thinkers such as John Locke, Joseph Raphson, and Newton.[20] The next section argues that there are also deep similarities between Clarke's views and More's, although Clarke does not acknowledge them.

[17] More (1995, I.viii.13, p. 1:60). [18] More (1995, I.iv.4, p. 1:27).
[19] More (1995, scholia to I.viii.13, p. 1:67).
[20] On More's account of space and its influence more generally, see Reid (2012); Leech (2013); Thomas (2018).

Clarke: Space as an Attribute of God

The famous Newtonian Samuel Clarke (1675–1729) argues for the divine nature of space in his first set of Boyle Lectures, delivered in 1704 and published in 1705 as *A Demonstration of the Being and Attributes of God*. There, Clarke writes that "Infinite Space" and "Infinite Duration" are "Attributes of an Essence Incomprehensible to Us" – God.[21] This position is akin to that of More.

Clarke famously defended Newton's views from objections by Leibniz in a 1715–1716 series of letters, first published in 1717. In his side of the correspondence, Clarke seems to advance the same view as in his *Demonstration*. In his fourth letter to Leibniz, dated June 26, 1716, Clarke writes that extramundane space "is not *imaginary*, but *real*," and "*Space* void of Body, is the Property of an *incorporeal* Substance."[22] Although Clarke now argues that space is a "property" of God, rather than an "attribute," he does not seem to mean anything by this shift (and in writings before and after, he uses the terms 'property' and 'attribute' interchangeably).

Clarke emphasizes that, on his view, space is a property of a substance, not a substance itself:

> Space *is not* Any kind *of* Substance . . . *Because* infinite Space *is* Immensitas, *not* Immensum; *whereas* infinite Substance *is* Immensum, not Immensitas . . . *It remains therefore, by Necessary Consequence, that* Space *is a* Property.[23]

For Clarke, God is the immense (*immensum*). Infinite space is not the immense, it is immensity (*immensitas*). Space is identified with God's attribute of immensity, not with God himself.

These kinds of statements appear to distinguish Clarke's view from that of More, who occasionally writes that space is God's substance. Arguably, however, there is no difference between them. More draws a conceptual distinction between God and space, not a real distinction. The same is true of Clarke. Since Clarke holds that space is an attribute of God's substance, and that there cannot be a real distinction between God and his attributes, in reality space is the substance of God.

Newton: Space as Independent

Clarke and Newton were colleagues and friends, and when Clarke exchanged letters with Leibniz in 1715–1716, it was widely assumed that Clarke was acting

[21] Clarke (1705, §4, pp. 79–80). [22] Clarke and Leibniz (1717, Fourth Reply §§7–8, pp. 125–127).
[23] Clarke and Leibniz (1717, Fifth Reply §§36–48, p. 307).

on Newton's behalf. Leibniz himself wrote of their letters, "I am now grinding a philosophical axe with Newton."[24] Throughout the eighteenth century, philosophers working in various contexts – such as Émilie Du Châtelet (1706–1749) in France in the middle of the century and Immanuel Kant (1724–1804) in Prussia toward its end – regarded Clarke's famous exchanges with Leibniz as accurate representations of Newton's ideas about space, time, motion, and much else besides. Some modern scholars still read Clarke as a mere mouthpiece for Newton's views.[25] However, Clarke's *Demonstration* came several years before Newton published his own views on the relationship between God and space in the 1706 Latin translation of his *Opticks*, and in the second, 1713 edition of the *Principia*. Also, nowhere in Newton's writings is space described as a property or attribute of God. This raises doubts about identifying Clarke's side of the Leibniz–Clarke correspondence as an expression of Newton's own views.[26]

More than anything, it was Newton's understanding of space that Leibniz found objectionable, especially the view of so-called absolute space presented in the Scholium following the Definitions that open his *Principia*, first published in 1687 (Leibniz took notes on the text, sent to him by Newton, in 1688). Newton's discussion in that text of absolute, mathematical, and true space – as opposed to relative, common, and apparent space – set the terms for thinking about space, time, and motion for the next three centuries. Indeed, it continues to shape philosophical debate to this day. Throughout most of that time, Newton's view has been contrasted with Leibniz's claim that space and time are not absolute but merely relative.

Although most interpreters of the Scholium in the *Principia* have read the text and its arguments through the lens provided by the Leibniz–Clarke correspondence, it was actually written long before Newton had focused on Leibniz and his followers. Instead, the principal conception of space and motion contested by Newton's text is the view outlined in Parts II and III of Descartes's *Principles of Philosophy* (1644). Indeed, Newton paraphrases Descartes's *Principles* in places and expends considerable effort trying to undermine the Cartesian understanding of true motion. So Newton's view should first and foremost be contrasted with Descartes's.

[24] Newton (1976, 355).

[25] For example, Vailati (1997, 109) writes that Clarke's account of space and time is "virtually" identical to that of Newton.

[26] For more on Clarke's account of space, see Ferguson (1974); Vailati (1997); Thomas (2018, 156–181).

In Part II of his *Principles*, Descartes had distinguished between what he calls the *vulgare* or common conception of motion, according to which motion involves a change of place, and the proper conception, according to which motion involves a change of an object's "vicinity." Hence for Descartes, properly speaking, the motion of an object is a function of its changing relationship with the bodies that surround it (its vicinity) – motion is relative in that precise sense.[27]

Newton objects to this view because it severs the connection between true motion and its causes, a connection that is evident already in Descartes's own laws of nature. Consider what came to be known as the principle of inertia, a version of which is expressed in Descartes's first two laws: a body moving rectilinearly will continue moving in that way until it is caused to do otherwise (just as a body at rest will remain at rest until it is moved by some cause). This implies that a body's true motion is *not* a function of its relationship to its vicinity – a body moving rectilinearly cannot, for example, be brought to rest by surrounding it with particles so that they constitute a vicinity from which it is not transferred, as Descartes's proper conception implies. Since the law of inertia suggests that true motion is not a function of a body's changing spatial relationships with other bodies, one should realize that true motion is not a species of relative motion. Newton suggests instead that true motion is *absolute* motion. This last idea, of course, requires the notion of absolute space, the space in which all true motion takes place. In this way, Newton's conception of absolute space reflects what he takes to be a lesson from the laws of nature; namely, that true motion is not a species of relative motion. But the idea of absolute space was one of the most controversial in all of eighteenth-century natural philosophy.

It easy to see why the controversy began almost immediately. In order to articulate his view that true motion is absolute motion, Newton presented the idea that "absolute space, of its own nature without reference to anything external, always remains homogeneous and immovable."[28] Thus, space is homogeneous and immovable independent of anything else (arguably with the exception of God). Consequently, Newton's readers thought he had conceived of space as a substance of some kind, for he had attributed two

[27] More precisely, Descartes says that, properly speaking, motion is the transfer of a body from its vicinity, which is regarded as being at rest, to a new vicinity. Paraphrasing Descartes's *Principles*, Newton sums up his objection to the Cartesian conception as follows: true motion "cannot be determined by means of change of position from the vicinity of bodies that are regarded as being at rest" (Newton 2004, 67).

[28] Newton (1999, 408–409).

basic features to space and contended that it had those features independently of other things. As they quickly argued, space is a strange kind of substance: it is causally inert, which renders it imperceptible. For many early moderns – including, ironically, Newton himself and certainly Leibniz[29] – a substance is inherently something that is active. Many doubted the coherence of the idea that space is both independent of everything else and the subject of predication, on the one hand, and inherently inactive, on the other hand. It seemed like a substance that is not really a substance. Newton's further implication that this quasi-substance is also *infinite* only served to muddy the waters. It brought Newton perilously close to saying there is something other than God that is independent of all matter, eternal, and infinite.

Newton knew very well that his conception of space raised difficulties that would be on the minds of many philosophers encountering his view. It therefore remains somewhat perplexing that Newton chose not to adhere to the strategy used in his unpublished anti-Cartesian tract, called *De gravitatione* after its first words, in the published text of the *Principia*. Whereas he expends considerable effort in *De gravitatione* to show the coherence of the contention that space is neither a substance nor an accident, and indeed that its relationship with God is not worrisome, he simply ignores these general philosophical issues in the *Principia*.[30] The Scholium to the Definitions does not broach the question of how to think about absolute space through the lens of a substance-based metaphysics. Although the General Scholium briefly discusses God's substantial omnipresence throughout infinite space, a view Clarke also defends, Newton there, too, ignores the philosophical problems arising from his view. The divine being is present substantially throughout space, but is space then a divine attribute, or a substance in its own right? If the former, is that not a problem? And if the latter, does natural philosophy really have room for two eternal and infinite substances? We know that Newton took these questions seriously in private because he discusses them in great depth in *De gravitatione* after explaining his differences with Descartes, but he largely refrained from answering them in public. His

[29] Newton (2004, 21–22). For his part, Leibniz clearly thinks that substances must be active – see, e.g., the opening of the *Specimen dynamicum* of 1695 (Leibniz 1989b, 118).

[30] In *De gravitatione*, Newton insists that space has its own manner of existing; that is, he denies that a philosopher is forced to think of space as either a substance or a property of some other substance, or else as nothing at all (Newton 2004, 21). To clarify his view, however, he decides to follow an element of the Cambridge Platonic view of space as an emanation from the first existing being (a view one might find in More), but the view is not easy to understand. See the debate between Carriero (1990) and McGuire (1990) on how best to understand it.

silence did not quiet the philosophical uproar; nearly every major post-Newtonian philosopher, from George Berkeley to Du Châtelet to Leibniz to Christian Wolff to Kant, rejected absolute space on philosophical grounds.

The basic problem with understanding Newton's conception of space could not easily be escaped, since it arose directly from the physical laws of nature yet conflicted with the common understanding of substance, and Newton (publicly) proposed no new metaphysical principles to resolve the tension. In fact, the difficulties posed by Newton's conception of absolute space were still present at the end of the eighteenth century, when Kant complained in his *Critique of Pure Reason* that space and time for Newton are "Undinge" or non-entities.[31] The problem, as Kant and others realized, was to find a coherent and useful alternative to Newton's view.

Leibniz: Space as Relations

Although Newton articulated his view of space by thinking through the problems with Descartes's, it was eventually Leibniz's view that became the most important alternative to Newton's.[32] Before he presented his alternative view, Leibniz frequently reminded Clarke of the problems with Newton's view. Leibniz calls "real absolute space" an "idol" of some modern English philosophers – using the famous term of Bacon's – and then notes that it seems "such a being must needs be eternal and infinite."[33] Certainly, if space is independent of matter, then it is not contingent upon any material body, and for that reason might be considered eternal. To add insult to injury, Leibniz then reminds Clarke that if absolute space is eternal and infinite, it becomes difficult to understand its relation to God. Must one admit that space simply *is* the divine being, or perhaps one of God's "attributes"?[34] As noted above, such problems dogged the Newtonian view throughout the eighteenth century, and Newton's public silence did not help.

But Leibniz's attack does not rest there. Instead, his most famous arguments against Clarke's understanding of space employ a principle that Newton himself ignored, namely, the principle of sufficient reason (PSR), the tenet that there is a sufficient reason why every individual thing is how it

[31] Kant (1998, A39/B56, pp. 183–184).
[32] The literature on the Leibniz–Newton debate concerning the nature of space and time is voluminous. See, e.g., the influential treatments in Sklar (1974), Earman (1989), and Vailati (1997). On broader disagreements between Leibniz and Newton, see especially Bertoloni Meli (1993).
[33] Clarke and Leibniz (1717, Third Paper §3, p. 57).
[34] Clarke and Leibniz (1717, Third Paper §3, p. 57).

is rather than otherwise. The classic form of his argument appears in the fifth section of his third letter to Clarke, and runs roughly as follows. If space is absolute, it is independent of matter, and if it is independent of matter, then it must have existed prior to the creation, which means that God faced the question of *where* to place created matter. However, since space itself is uniform – Newton says "homogeneous" – there could not be a reason for God to choose one region of space rather than another to place created matter. Anything that would count as a reason to place created matter "here" would equally count as a reason to place it "there." Therefore, either God did not create matter, which is anathema, or did so without reason, which violates the PSR. One can escape this dilemma by jettisoning its presupposition that space is absolute. If we follow Leibniz and think of space as nothing but the order of relations among material bodies, then there is no meaningful question as to where God ought to place created matter – to create matter is *ipso facto* to create an order of relations among material things, and therefore to create space itself. Problem solved.

For his part, Leibniz spent little time in his exchange with Clarke explaining the nuances of his own view, focusing instead on indicating how it evades the many difficulties Newton faces. But Leibniz certainly was prepared to explain them. On its face, the idea that space is just the order of relations amongst material objects seems problematic, for it seems to imply that if we have a finite amount of matter, then we are stuck with a space that is coincident with the outer bounds of that matter. That seems odd in at least one way: surely some material body that happens to be on the edge of the material universe could move further away from the bodies nearest to it, at least, as a matter of metaphysics (there may be *physical* reasons that it cannot do so that need not concern us here). Leibniz would agree. His considered view is arguably that space is not just the order of actual relations, but the order of actual and *possible* relations.[35] The furthest material body has actual relations with its neighbors, but could move further away from them because that would involve its *possible* relations.[36] In this way, independent of the size of the actual world of material objects, Leibniz's space can be arbitrarily large. (Whether it can be infinite is a separate question.)

The idea of space as a relational order enables Leibniz not only to evade problems with the PSR, as he insists to Clarke, but also the more general

[35] See, e.g., Clarke and Leibniz (1717, Fourth Paper §41, p. 113). Belot (2011, 173–185) shows nicely why we should regard Leibniz as a modal relationalist.

[36] Leibniz apparently did not contemplate the even more abstract view of the twentieth century that space may involve actual and possible *relations* and *objects* – see Sklar (1974).

metaphysical problems concerning substancehood that seem to plague Newton's view. For Leibniz thinks that it is a mistake to regard space as a being or substance in any sense. Space depends on matter in the sense that it simply *is* the order of relations amongst material bodies. There is therefore no temptation to regard it as a substance, as an independently existing being. To take a favorite example of Leibniz's, just as it is a mistake to think of the Prussian army as a being distinct from the soldiers and horses and bayonets that constitute it, it is a mistake to think of space as a being distinct from all the relations among material bodies that constitute it. One has an army just in case one has soldiers and horses and bayonets, and one has space just in case one has material bodies and relations among them. This deflationary view was very influential in part because it seemed like a nice means for avoiding Newton's problems.[37]

Cockburn: Space as a Third Kind of Substance

Catharine Cockburn (née Trotter) (1679–1749) was an English philosopher once described by Toland as an "absolute Mistress of the most abstract Speculations in the Metaphysics."[38] She is best known for defending some of Locke's epistemological views, but also published on metaphysics. This section will consider her "Cursory Thoughts" prefixed to her 1743 *Remarks Upon Some Writers on Morality*. Although Cockburn nominally defends Clarke's account of space, in fact her position is quite different. Her critique of Descartes leads her to a unique metaphysic, one so unusual that it warrants inclusion amongst our other, better known accounts of space. Cockburn advances several arguments for realism about space, including the empiricist claim that, like the idea of matter, the idea of space is imposed on the senses. If we accept the evidence of our senses regarding matter, we should accept it for space too.[39]

We know that Cockburn holds space to be real. What is it like? Cockburn agrees with Clarke on many of the features of space: it is incorporeal, indivisible, implicitly partless,[40] and it possesses a kind of infinity.[41] However, as Cockburn's full argument for space reveals, she fundamentally disagrees with Clarke over the nature of space.

[37] See, e.g., the helpful discussion in Earman (1989, ch. 1), which is on the origins of the modern debate about space and time in the debate between Leibniz and Newton. He also explains Leibniz's influence on modern-day relationists.
[38] Toland (1704, preface). [39] Cockburn (2006, 95–97). [40] Cockburn (2006, 98).
[41] Cockburn (2006, 104).

One of her starting points for realism about space is Cartesian dualism, the belief that two kinds of substance exist, material and immaterial. Another is the metaphysical view eponymized as the Great Chain of Being, on which every possible kind of being exists in a hierarchy, leading upward from rocks, through plants, animals, humans, and spirits, to God. On this "chain" of beings, there is "gradual progress in nature," so the most perfect example of an inferior species comes very near to the most imperfect example of the superior species above.[42]

Cockburn combines dualism with the Great Chain to produce a novel argument concerning the existence and nature of space. She argues that dualism is an incomplete metaphysic – something must stand between material body and immaterial mind:

> there should be in nature some being to fill up the vast chasm betwixt body and spirit; otherwise the graduate would fail, the chain would seem to be broken. What a gap between *senseless material*, and *intelligent immaterial* substance, unless there is some being, which, by partaking of the nature of both, may serve as a link to unite them, and make the transition less violent? And why may not space be such a being? Might we not venture to define it, *an immaterial unintelligent substance, the place of bodies, and of spirits, having some of the properties of both*.[43]

The idea is that body and soul do not differ from each other by sufficiently gradual degrees, so we must posit an additional kind of substance to fill the gap: space. On this view, space is a unique kind of substance, distinct from matter and from God. As such, Cockburn's position is quite unlike that of any of her peers, including Clarke – whose view she is supposed to be defending. In the course of defending Clarke, Cockburn has developed a position on space that is wholly her own.[44]

Conclusion: Common Ground on the Features of Space

We have seen that early modern thinkers advanced radically different alternatives in the course of rejecting Descartes's view of space. However, in addition to wondering what space *is*, one might also ask, what are the basic features of space? That is, leaving aside the question of space's relation to

[42] In the early modern period, this idea was held by many philosophers, including Ralph Cudworth and Locke. Lovejoy (1936) provides a book-length exploration of this idea.
[43] Cockburn (2006, 97).
[44] For further discussion, see Broad (2003, 158–163); Thomas (2013).

matter, or to anything else, including the divine being, what features might we predicate of space? Intriguingly, once we ask this question, we find commonalities underlying the debates. For example, More, Clarke, Newton, and Cockburn agree that space is uniform or homogeneous, the same everywhere. Importantly and perhaps surprisingly, Leibniz also holds this view. In the course of arguing against Newton and Clarke, Leibniz writes: "Space is something absolutely uniform, and without the things placed in it, one point of space absolutely does not differ in any respect whatsoever from another point of space."[45] The double use of 'absolutely' may signal to the reader that Leibniz is simply stating an assumption or aspect of the Newtonian view here, but in fact he is not. For the very next thing he says is, "Now from this it follows (supposing space to be something in itself, besides the order of bodies among themselves),"[46] which means that he has not supposed Clarke's view until this later step in the argument. This point is often overlooked, and it means that for Leibniz, space's existence and its features are distinct – only the former, but not the latter, depend on matter. Space exists just in case bodies exist; but if bodies exist, then what also exists is something absolutely uniform. And it should be obvious, in turn, that space's *uniformity* does not supervene on bodies (it depends on bodies only in the sense that it is a feature of space and the latter depends for its existence on bodies). After all, bodies are discrete, made of parts, movable, and not uniform; they and their relations give rise to something that is absolutely uniform, continuous, immovable, and so on. In sum, the world could be arbitrarily different than it really is as far as the features of all the material bodies that exist are concerned, and space would still exist and remain absolutely uniform. That is normally regarded as expressing Newton's view, but it also expresses Leibniz's – a surprising area of agreement between them.

To understand the details of this agreement, let us return to Leibniz's army analogy. One has an army, presumably, only if one has soldiers and bayonets and horses; if one has a pile of marbles, then one might have a game, but not an army. So the higher-level entity depends on the features of the lower-level entities, as one would expect. That is *not* the case with space – once one has *any* set of material objects, whether an army or a pile of marbles or any set of two arbitrarily chosen objects, then one has space, and one can alter that set of objects in any arbitrary way without altering space's uniformity in the

[45] Clarke and Leibniz (1717, Third Paper §5, p. 59).
[46] Clarke and Leibniz (1717, Third Paper §5, p. 59).

slightest (as long as one does not destroy the objects). It is obvious that Newton – along with More, Clarke, and Cockburn – thinks of space as unique. But on reflection, it also seems that Leibniz thinks of it as unique. Although he denies the independence afforded it by Newton, he contends that space's features do not supervene on the features of the objects on which it depends. Because this commonality between the views of Leibniz and Newton and the others is easy to miss, its philosophical significance has not been plumbed.[47] It certainly seems to suggest that despite their deep differences, Leibniz and Newton concur that space (along with time) has a kind of special status. In insisting that space is independent of matter, even as its status as a substance or property is unclear, Newton places space into a special category. And it can seem at times that Leibniz is striving precisely to *avoid* placing space into that category. Yet at a deeper level, Leibniz agrees that space has a special status: although its existence is contingent, its features are not.

This surprising underlying agreement leaves us with a pressing question: why does space have a special status? Why should it be the case that it has certain features regardless of what exists? It is precisely this kind of question that leads Du Châtelet to characterize space's special status in Chapter 5 of her *Institutions physiques* in 1740. By century's end, Kant had decided to give space and its representation pride of place in the first major section of the *Critique of Pure Reason* (1781). For Kant, it is precisely an analysis of the special status of space – which he thought neither Leibniz nor Newton nor any other thinker fully appreciated – that leads one to the philosophically revolutionary position of transcendental idealism. That position, in turn, brought the discussion of space and time to a new level of philosophical prominence and sophistication.

[47] Perhaps Belot comes closest to making something like this point: "It seems clear here that space has the same structure at worlds in which matter is sparse as it does at worlds in which it forms a plenum with the structure of Euclidean space" (Belot 2011, 177). That is, Leibniz thinks that the structure of space is what it is independent of matter. It does not matter what matter is like, or as Belot says, it does not matter whether there is a plenum or not, because space's structure is invariant. So, for Leibniz, space's structure is a special feature of reality in just the sense that it is invariant with respect to the extent and properties of matter, an idea we would normally associate with Clarke and Newton.

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The *Vis Viva* Controversy

ANNE-LISE REY

When two bodies collide and then continue moving after impact, what is conserved?[1] Is it a physical quantity? Can it be measured? For René Descartes and his followers in the seventeenth and eighteenth centuries, the reply to these questions was that the total *quantity of motion* of the bodies is always the same before and after the collision, where each body's quantity of motion can be measured as the product of its size and its scalar speed (mv).[2] If one body ever loses motion, that motion must have been transmitted to another body. This law of the communication of motion guarantees that the total quantity of motion in the world is constant; a constancy Descartes founds upon divine immutability. Isaac Newton and his supporters also held that quantity of motion is the quantity conserved in collisions, but thought it necessary to consider the direction of the motion, as well ($m\boldsymbol{v}$). Gottfried Wilhelm Leibniz, on the other hand, defended the idea that when two bodies collide, what is conserved in the universe is the quantity of *force* – i.e., *vis* (*viva*). This is conceived as a quantity measured by the product of the mass and the square of the speed (mv^2). Leibniz restricted the application of his conservation principle to elastic collisions, but within that constraint, the Leibnizian and Cartesian principles could be seen as predictively equivalent for collisions. Consequently, what was at stake in the quarrel was not the veracity of the competing calculations, but the metaphysical presuppositions carried with them. That is, the dispute put into contention not only differing estimates of the conservation principle, but also – and perhaps even more importantly – differing conceptions of mechanics.

But this long-running dispute also raises a crucial question for the historian of scientific ideas: what are the reasons and procedures for adopting a scientific theory? By way of answer, this chapter considers how, in the quarrel, each

[1] See Bertoloni Meli (1993); Duchesneau (1994); Fichant (1998); Rey (2016).
[2] See *Principles* II.43. For difficulties with this quantification, see, e.g., Slowik (2017). Also Shea (1991); Garber (1992); Gaukroger (1997); McLaughlin (2000).

position regarding the principle of conservation responded to distinct epistemic needs, and thus how various epistemic communities confronted the issue, including their reflections on the status of laws of nature and their methodologies for acquiring certain knowledge about nature.

The morals of this controversy are twofold. First, it shows that the resolution of controversies goes beyond consideration of the strength of the arguments offered by opposing camps. In other words, it is not certain that "the best argument" makes one side win – rational consensus may not determine the outcome. But neither is it clear that institutional and contextual reasons are decisive. What follows illustrates how, as in any controversy, an antagonistic irreducibility draws or reconfigures epistemic divisions. This is why, beyond the question of calculation and the opposition between motion and force, the *vis viva* debate features an opposition between epistemic regimes; in this case, different ways of reconciling experimental proof and metaphysical principle. This chapter will trace the epistemic consequences of such divergent methodological proposals and the philosophical consequences they have for the interpretation of nature.

This leads, in turn, to the second moral. The epistemic and methodological aspects of the controversy impact the very conception of the Scientific Revolution. The length and posterity of the quarrel offer rich grounds upon which to test the canons and criteria of the Scientific Revolution. The astronomical paradigm of the Scientific Revolution is the famous (and almost impossible to isolate) transition from geocentrism to heliocentrism; while in physics, it is often associated with the mathematization of natural phenomena. But equally important has been the conception of the Scientific Revolution as the emergence of a science emancipated from metaphysical foundations and justified exclusively by recourse to experiments, encapsulated in the famous Newtonian dictum, *"Hypotheses non fingo."* Against this, the hypothesis guiding this chapter is that the successive positions adopted after Newton consisted in constructing new and divergent articulations of metaphysical principles in addition to experimental practices. Consequently, metaphysics was not excluded from the new science, but circumscribed and sublimated – relegated, perhaps, to a darkened corner of the laboratory, it did not entirely go away.

The First Quarrel over Living Forces

The quarrel over living forces – in its first phase – began with Leibniz's *Brevis demonstratio erroris memorabilis Cartesii*, published in the *Acta eruditorum* in 1686.[3]

[3] Leibniz (1962a).

For Leibniz, the aim was to establish two theses: on the one hand, that the principle of conservation of the quantity of motion defended by Descartes in the *Principles of Philosophy* was wrong, and should be replaced by the principle of conservation of *vis viva*; and on the other hand, that it was possible, despite recent discoveries by Christiaan Huygens, Christopher Wren, and John Wallis tending to instrumentalize the laws of collision, to maintain a conception of a law of nature as a theoretical principle that organizes and regulates phenomena.[4] Indeed, the problem raised by the new theorization of physics in the *Brevis demonstratio* is that of elucidating the relationship between the explanation of physical phenomena and divine universal legislation. In Leibniz's terminology, the question is how to think about the relationship between "subordinate maxims" or "laws of nature," on the one hand, and the "system of the world" that orders the unified whole of things, on the other. Ultimately, this question is answered by the demonstration of the principle of conservation of *vis viva*.

Leibniz interprets the relationship between the general order and the phenomenal laws as a subordination, which can be understood in two ways. First, the divine understanding has identified and chosen the best among compossible worlds, thereby enacting a system of nature. The legislation at work in this general order thus contains all the determinations expressed in the development of each created substance. It governs both what finite substances are, since they are the expressions of the general order at the level of phenomena, and what appears in our experience as interactions between bodies, but which are really expressions of the intrinsic harmony of this optimal world. Consequently, the rules that describe phenomena are subordinate to the general order in that they are determined by it. The second way of understanding the subordination is by noting that these rules – the laws of nature, which are essentially laws of motion and interactions – are learned merely from experience by an *a posteriori* elaboration.[5] This implies that the reason governing the general order transcends what is provided by experience, which merely reflects this reason.

What must be precisely understood here is the distinction and correlation between two terms that refer to different levels of expression. Article 17 of the *Discourse on Metaphysics* shows the three levels at which Leibniz apprehends force. At a metaphysical level, we can identify the "motive" or "driving" force, which is present in bodies as an expression of the general order of the

[4] See Leibniz (1989a, §18, pp. 51–52); Duchesneau (1994, 137–138). This interpretation is elaborated against, for example, Iltis (1971); Brown (1984).

[5] See Gueroult (1939).

universe – it is what constitutes a body as a body. At the physical level of observable phenomena – in other words, at the level of the subordinate maxim – the motive force is translated into the "living force" preserved through bodily interactions. Finally, at a third level, the mathematical expression mv^2 represents the living force. In sum, the "motive force" is a general theoretical entity that is preserved, while the "living force" interprets the theoretical entity at the level of physical phenomena, making it possible to observe this theoretical entity and symbolize it by the formula mv^2.

Compare this to Descartes's theory. At the metaphysical level, Descartes appeals to the perfection of God as a cause and to the conception of corporeal reality as reducible to extension; e.g., in Article 36 of Part II of the *Principles of Philosophy*: "That God is the primary cause of motion; and that He always preserves an equal quantity of it in the universe."[6] Together, these entail at the physical level the conservation of quantity of motion – i.e., the conjunction of extension and speed – represented in turn by the expression mv.

A Posteriori Demonstration of the Principle

To explain the status of laws of nature and their double subordination, Leibniz uses, in the *Brevis demonstratio* and in Article 17 of the 1686 *Discourse on Metaphysics*, what he calls an example, but which is not just any example: the Cartesian principle of conservation of the quantity of motion. Leibniz's project in these two texts is a matter of (1) positing the conservation of a theoretical entity – motive power in the *Brevis demonstratio* and force in the *Discourse on Metaphysics*[7] – and (2) denouncing Descartes's assimilation of motive force with quantity of motion, and proposing to replace it by a correlation between motive force and living force.

Leibniz's *demonstratio* of Descartes's error is based on an initial agreement with two Cartesian premises. First, a body falling from a certain height acquires the force to climb up to the same height, if it is not prevented from doing so by an external impediment. This premise was implicitly present in Galileo's mechanics, and was explicitly formulated by Huygens.[8] The second premise is that it takes as much force to raise a one-pound body to a height of 4 *toises* as it does to raise a four-pound body to a height of a *toise* (which corresponds to about 2 meters). Descartes had stated this equivalence in his *Traité de mécanique* and in an appendix to a letter sent to Huygens on

[6] Descartes (1964–1974, 8:61); Descartes (1982, 57).
[7] Leibniz (1962a, 117); Leibniz (1989a, §17, pp. 49–51).
[8] Huygens (1669); Galilei (1989, 206).

5 October 1637. Both the *Brevis demonstratio* and the *Discourse* then introduce Galileo's empirically derived law according to which the distance fallen is proportional to the square of velocity.[9] "All this is admitted by our new philosophers," Leibniz writes, referring to the mechanistic scholars close to Descartes.[10]

At this point, Leibniz compares the falls of two bodies, of the one-pound *A* from a height of 4 *toises* and of the four-pound *B* from a height of 1 *toise*. By the Galilean law of free fall, the quantity of motion acquired by *A* is expressed as the product of 1 pound and the square root of 4 *toises* – 2 – while the quantity of motion acquired by *B* is 4 *toises* times the square root of 1 *toise* – 4. But by the second premise above, the force required to raise these bodies is equal, so by the first premise, they should acquire the same force at the end of their fall. The Cartesian calculation thus yields a contradiction – but this invalidates the conservation of quantity of motion that entails that calculation. Leibniz concludes, "there is a great difference between quantity of motion and [motive] force."[11]

In favor of his own position, Leibniz points out that the conservation of a quantity expressed as mv^2 comports with Galileo's empirical law, since it generates the expected equivalence in the falls of *A* and *B* – the calculation yields a magnitude of 4 for both. To this, Leibniz adds that his view also comports with an *a priori* principle of equivalence between the full cause and the whole effect. In this case, the motion by which the moving body returns to its starting point after its fall must exhaust all the force that had been produced by the fall itself. Leibniz undoubtedly found this principle in Wallis's *Mechanica* (1670–1671), where one reads, "the effects are proportional to their adequate causes."[12] But Leibniz, as early as the 1676 *Catena mirabilium demonstrationum de summa rerum*, ascribes this equivalence to the principle of sufficient reason:

> Nothing is without cause, because nothing is without all the requirements for this thing to exist. The entire effect is equivalent to the full cause, because there must be an equation between cause and effect, ensuring the transition from one to the other. However, it consists of this equivalence, and no other measure can be found.[13]

So it is that, by the end of the *Brevis demonstratio* and Article 17 of the *Discourse*, Leibniz has rejected the Cartesian principle in favor of a conservation of motive force expressed as mv^2 based on both what experience allows us to

[9] Galilei (1989, 209). [10] Leibniz (1989a, 50). [11] Leibniz (1989a, 51).
[12] Wallis (1670–1671, 1:15). [13] Leibniz (1948, 263).

observe and what reason teaches us about the equivalence of cause with effect. Note that this illustrates the second subordination noted above – that the lessons of experience are secured by transcendent reasons.

Metaphysical Considerations in the Recalculation

But what of the first subordination? How do the "laws of nature" express the legislation of the "system of the world"? Article 18 of the *Discourse* – instructively titled "The distinction between force and quantity of motion is important, among other reasons, for judging that one must have recourse to metaphysical considerations distinct from extension in order to explain the phenomena of bodies"[14] – shows how *vis viva* makes intelligible the relationship between physical phenomena and the metaphysical legislation governing them.

At the physical level, phenomena can be described as mechanical interactions and expressed mathematically. But the ultimate explanation for the behavior of bodies is the principle of order underlying the phenomena, which grasps them not singularly, but as a whole – in other words, as the general structure of the universe. According to Leibniz, this general structure is applied to the natures of individual bodies via their substantial forms, which individuate each body, but nevertheless harmonize with all the others. Leibniz's rehabilitation of substantial forms thus makes the substantiality of each body depend on something beyond geometric characterization – bodies *are* something separate from extension and motion.[15] But the harmonization of forms also legislates what bodies *do*; it entails the motive force by which they act, expressed by *vis viva* in the phenomena. We therefore understand the relationship between substantial form and force. They are two apprehensions of the same reality – substance – at different levels of intelligibility, and captured in distinct dimensions. Substantial form accounts for substance at the metaphysical level; *vis viva* accounts for substance at the physical level. In both cases, one is speaking of an internal, incorporeal principle at work in bodies. Force makes it possible to articulate the levels of intelligibility instead of merely distinguishing them, if we understand the relationship between metaphysical force and physical force. Leibniz continued working on the conceptualization of force with the invention of his dynamics, in *Dynamica de potentia et actione* (1689–1690), which remained unpublished until the 1860s.

[14] Leibniz (1989a, 51). [15] See Leibniz (1923–, 1-1:225); Leibniz and Fichant (1993).

We can now comprehend Leibniz's additional *a priori* argument that, against Descartes, force is "more real" than motion. To the extent that motion is assimilated to a simple translation, a displacement relative to a reference point, it cannot be attributed to a body in its own right. It is an extrinsic relation between the moving body and the reference system. In this sense, it must only be a phenomenon, and not a real property embedded in the substantiality of a thing. Conversely, force is an absolute, because it is the internal principle of displacement, irrespective of relative place. As noted above, at the physical level, distinguishing *vis viva* from quantity of motion makes it possible to assert accurate natural laws and rules of collision (thereby correcting Descartes's errors). Yet at the metaphysical level, the force is anchored to the doctrine of substance, while motion is cast adrift.

It is worth stressing, though, that the issue at stake in the quarrel is less the accuracy of an estimate that may or may not be validated by experiments than the general conception of physics, the status and meaning it gives to the laws of nature and the relationship it establishes with metaphysics. It is therefore understandable that in the repertoire of arguments mobilized to defend one conservation principle against another, experimental evidence functions at worst as a misinterpretation of the issues in dispute, at best as a step in the process of resolving them.

The Second Quarrel over Living Forces

A simple reading of the texts would lead us to date the beginning of the dispute to Leibniz's *Brevis demonstratio* of 1686. Yet, in Jean le Rond D'Alembert's opinion, which is followed by the main commentators, the quarrel over living forces did not really begin until 1728,[16] more than forty years later, with the "Essay on the Estimation and Measurement of the Body Motive Forces," presented to the Parisian Académie des Sciences by Jean-Jacques Dortous de Mairan, though almost unanimously supposed to be the work of the Académie's then Perpetual Secretary, Bernard Fontenelle.[17] How can we understand this? A key context is the Académie des Sciences itself, which provided an institutional setting for the debate, partly through the prize competitions it sponsored.[18] But this is not only a question of documenting the official chronology of the polemic, so to speak, but also, and perhaps above all, of understanding how the opposition between Descartes

[16] D'Alembert (1758, xvii). [17] See Costabel (1983, 37), but also Iltis (1971).
[18] Terrall (2004).

and Leibniz about conceptions of matter and the calculation of physical quantities was transformed forty years later into a dispute between two epistemic regimes: the one embodied by Leibniz, the other developed by Newton. We need to understand how the epistemic model shaped by Newton became the main target of the supporters of Leibniz.

Pierre Costabel, in a dossier dating to 1983 and still authoritative today,[19] traces the main stages of the dispute in the dual light of the spread of Leibnizian dynamics and of the controversy with Newtonians over the invention of differential and integral calculus, recording these elements in the milieu of the Académie des Sciences and in the questions it tendered. It is worth recalling that the quarrel acutely broke out after the Académie, having posed a question about the rules of collision of hard bodies, in 1724 awarded a prize to an essay by the Scottish Newtonian Colin Maclaurin, rather than the one by Swiss mathematician Johann Bernoulli, Leibniz's faithful friend and "critical supporter."

In Chapter 5, "On the Living Force of Bodies in Motion," of the *Discourse on Movement* he submitted to the Académie, Bernoulli writes about the dispute between conservation of quantity of motion and of *vis viva*. Here, Bernoulli imposes a stark difference between dead force and living force: "these are heterogeneous quantities that do not allow any comparison." Dead force is defined as a simple effort: "gravity produces a dead force in bodies whose effect is only momentary"; while living force, "whose nature is very different," is a pressure applied to a body that gradually gives it a local motion. The erroneous belief in the conservation of quantity of motion, he argues, "comes from the fact that the nature of dead forces has been confused with that of living forces," since it led to the mistaken application of the principles of statics, where dead forces preponderate, to the domain of moving bodies – i.e., dynamics – where *vis viva* operates. The equality Bernoulli establishes between efficient cause and effect leads him to affirm the conservation of forces.[20] In addition, he proposes two validations of the Leibnizian principle: one using experiments on soft matter or from the oblique collision of bodies; the other presenting a "general and geometric demonstration of the theorem of the quantity of living forces," as the title of Chapter 9 of the *Discourse* declares.

There may have been "objective" reasons why Johann Bernoulli did not win the competition. For one thing, he reformulated the terms of the question posed.[21] Nevertheless, Bernoulli's position raised a central problem

[19] Costabel (1983). [20] Bernoulli (1727, 31–38). [21] See Hankins (1965).

that troubled the prize committee: is it possible to adhere to the principle of conservation of *vis viva* without simultaneously adopting all the metaphysical considerations associated with it? In other words, is the Leibnizian principle irremediably inscribed in the scientific scheme of the late seventeenth century that presupposes a science founded in the nature of God, following the example of the Cartesian laws of motion based on divine immutability? Or is it possible to comprehend it simply as an instrumental rule for calculating expected outcomes, in the spirit of the Newtonian credo – namely, to use generalizations derived from experience while refusing to trace them back to any "hypotheses" on which they might be based? This issue is central to the posterity of the principle; indeed, Johann Bernoulli, Jacob Hermann, and Willem 's Gravesande pursued the latter option. But it is also central to the interpretation of Bernoulli's recalculation; for instance, in evaluating whether Bernoulli foreshadowed, once his theory was stripped of its "metaphysical slag," the conservation of energy.

The temptation to extricate the Leibnizian conservation principle from metaphysical considerations generated several inventive epistemologies during the Enlightenment. This was because attempts to make the principle a merely instrumental calculation rule had two important, if divergent, consequences: first, they focused on experimental proofs of the calculation; and second, they "discovered" a metaphysical need (apart from the Leibnizian one) for the conservation principle. The common feature of these schemes was their affirmation of the clear preeminence of experimental evidence without escaping metaphysical considerations. The major authors of eighteenth-century physics therefore engaged in an intricate dance – basing their conclusions on the irreducible use of experience, and at the same time trying to circumscribe the metaphysics in science.

The Reconfiguration of the Quarrel through the Prism of the Newtonian Epistemic Paradigm

One might follow the fortunes and misfortunes of the quarrel in the Académie des Sciences from 1728 onward to produce yet another history of the Leibnizian conservation principle in a context of conciliation with elements of Newton's natural philosophy.[22] Here, though, I will give two examples from contexts outside the Académie in France and the United Provinces. These will be useful to complicate the classic picture of

[22] E.g., Smith (2006a).

Enlightenment historiography, which makes the principle of conservation of *vis viva* disappear in the wake of the triumphant Newtonian dynamics.

A first example comes from experimental physics in the United Provinces. Gravesande is often presented as the introducer of Newtonian ideas to the United Provinces, a portrayal not contradicted by the title of one of his first books, *Introductio ad philosophiam Newtonianam* (1720).[23] During his stay in England between 1715 and 1717, he met Newton and was admitted to the restricted circle of his disciples, including Francis Hauksbee, John Theophilus Desaguliers, William Whiston, John Keill, and Roger Cotes, who had taken charge of the diffusion and simplification of Newtonian theses through the use of experiments or machines, in order to "make them sensitive" and thus more readily understood.[24] Appointed professor at the University of Leiden in 1717, Gravesande thus possessed all the qualifications necessary to officially introduce Newton's ideas in the United Provinces. And yet, in 1722 he published a *New Theory of the Collision of Bodies*[25] and, in 1723, an abridged version of his *Introductio*, the *Philosophiae Newtonianae institutiones*, wherein he significantly reduces the role of experiments, and – above all – introduces an explicitly Leibnizian chapter on the measurement of forces. As is widely known, this would be the starting point for an extremely virulent public controversy with Samuel Clarke, who accused him of having "betrayed" Newton.

The content of the dispute is interesting insofar as Gravesande holds that, by empirically demonstrating the laws of collision, he has behaved like a true Newtonian. That is, for him, the camp one belongs to is decided by a methodological choice, even though that choice leads him to assert the Leibnizian principle. Gravesande does not exactly follow the Newtonian method of crucial experiment, but devises variations of experimental conditions such that the conservation of *vis viva* is validated. Even though each individual trial is inconclusive, by varying the apparatus (copper balls sinking into clay, then ivory cylinders falling on a marble table), Gravesande reaches what could be considered an experimental proof. Thus, by producing empirical evidence for the Leibnizian principle, Gravesande develops an epistemology in which the laws of nature are discovered by experiments. On this view, the Leibnizian calculation is enlisted under the banner of a generally Newtonian methodology, and Gravesande proceeds to a Newtonian appropriation of the Leibnizian text. In his view, this is in no way an attack on Newton.

[23] Gravesande (1720). [24] Iltis (1973); Nyden (2014). [25] Gravesande (1722).

But the calculation includes the notion of force, which demands further explication. Consequently, Gravesande must countenance a metaphysical concept at the heart of his experimental practice, and he asserts a philosophical account of the concept. He writes, "the word Force expresses this power to act, which some Philosophers confuse with the Movement itself."[26] This is a metaphysical posit, not an experimental observation, yet it determines the interpretation of the experimental results. If Gravesande's experiment is an empirical validation of the Leibnizian principle, it depends on his reinterpretation of the complex relationship between speculative theory and experience. His metaphysical account shapes and corrects raw experimental results in order to give them meaning.

For his part, Clarke immediately understood the danger of adhering to the Leibnizian principle, which he was uncertain could be freed from its metaphysical dimension. By accepting the principle of force conservation, Clarke held, one was asserting a conception of body endowed with an active principle – on his view, an impermissible metaphysical "hypothesis."

The second example invokes a central figure in the Republic of Letters, Émilie Du Châtelet. Her position in the quarrel over living forces was made particularly visible by her participation in a famous controversy with Mairan, who had become Perpetual Secretary of the Académie des Sciences, and who rejected the arguments she used in favor of the Leibnizian principle in letters she made public and in her *Institutions de physique* (1740).[27] Du Châtelet has long been known almost exclusively for her masterful translation of Newton's *Principia*, but her engagement with Mairan offers entry into the novel natural philosophy she was developing. In contrast to Gravesande's grounding in experiment, Du Châtelet begins with metaphysics. On her view, Newton's contribution to physics is undeniable, but it cannot constitute an alternative model to the one shaped by Leibniz; it is simply a question of integrating Newton's physics into the Leibnizian, or rather Wolffian, metaphysics.

To oversimplify Du Châtelet's position, the principle of conservation itself is based on metaphysical principles, but the relevance of the principle to phenomena is based on experience. At the beginning of Chapter 21 of the *Institutions*, Du Châtelet takes up an argument that Leibniz developed in his texts devoted to dynamics.[28] She recalls the Leibnizian insistence on the equivalence of the full cause with the full effect, and reiterates the Leibnizian distinction between dead force and living force, repeating

[26] See Gravesande (1774, 257–258). [27] See Detlefsen in this volume (ch. 7).
[28] Whether we consider the *Dynamica de potentia* (1689–1690), the *Essay de dynamique* (1692), the *Specimen dynamicum* (1695), or the *Essay de dynamique* (1699–1700).

a comparison to the point and the line used by Leibniz, in which it is an integration procedure that makes the summation of the infinitely small dead forces the condition for generation of a living force.[29] Then, referring to Galileo, she reminds us that the spaces covered are proportional to squares of speeds and that, as forces are to spaces, forces are to squares of speeds.[30] But this calculation, she adds, has been demonstrated by many experiments – essentially, those of Gravesande and Giovanni Poléni – and is the consensus among philosophers. Ultimately, Du Châtelet ensures the validity of the Leibnizian conservation principle both with an elaborate metaphysical foundation and with experimental demonstrations.

The second part of Chapter 21 then responds to Mairan, who, in his *Dissertation sur l'estimation et la mesure des forces motrices des corps* (1728), had analyzed motions in general by reduction to uniform motions. This led him to consider, in estimation of the motive force, virtual trajectories not actually traversed by a body.[31] But, Du Châtelet points out, this runs afoul of the central tenet that causes are equal to effects. Mairan, she argues, has proposed to estimate the strength of a body "by what it does not do."[32] In other words, she contests his refusal to estimate forces by effects. Note, moreover, that this objection is metaphysical, not empirical.

Unlike Leibniz, Du Châtelet chooses to place her defense of the principle of the conservation of *vis viva* at a well-defined level of intelligibility – i.e., the phenomenal level – because she rejects the metaphysical conception of motive force.[33] This discrepancy must be qualified, however, by recalling that Chapter 21 develops the achievements of earlier chapters of the *Institutions* concerning the principles of sufficient reason and of continuity. In the end, it could be said that Du Châtelet, having placed the defense of the principle of conservation of *vis viva* in the general framework of abstract Leibnizian metaphysical principles,[34] chose to renounce Leibniz's abstract conception of motive force.

[29] Du Châtelet (1740, §566, pp. 419–420). See, for example, Leibniz's *Essay de dynamique* of 1692.

[30] Du Châtelet (1740, §567, pp. 420–423).

[31] This had already been the subject of a letter to Maupertuis in September 1738 (Kölving and Brown 2018, 1:378–381).

[32] "Réponse de Madame la marquise du Châtelet à la lettre de Mr de Mairan" (Du Châtelet 1742, 516).

[33] Du Châtelet (1740, §570, p. 424): "The question of the force of bodies ought not revolve around a metaphysical force without use and without resistance, for I do not know what the force is of that which never fights [*ne se bat point*]."

[34] See Rey (2007).

A key consequence can be drawn from this. Du Châtelet deploys a regime of dual proof that maintains a specific relationship to experience, while seeking also to rely on metaphysical notions, on the express condition that their respective fields of validity are clearly defined. The latter asserts the coherence and intelligibility of a theory, the former its applicability to the phenomena. This is clearly expressed in §588 of Chapter 21, devoted to the strength of bodies: "one has seen in this Chapter that all experiments concur in proving the living forces, but Metaphysics speaks almost as strongly as Physics in their favor."[35]

Conclusion: A "Dispute of Words"?

As is well known, D'Alembert, in the Preliminary Discourse of the second edition of the *Traité de dynamique* (1758), undertook to end the quarrel by reducing it to a "dispute of words."[36]

> If one proposes the same problem of Mechanics to two geometers, one of whom is an adversary, the other a partisan of the living forces, their solutions, if they are good, will always be in perfect agreement. The question of the measure of force is therefore *entirely useless* in Mechanics and even without any real object. It would not have produced so many volumes, if one had endeavored to distinguish between what was in it of clarity and what of obscurity.[37]

This point of view is taken up and confirmed in his later article on "Force" in the *Encyclopédie*:

> What we have just said about the famous quarrel of the living forces is taken from the preface to our *Treatise on Dynamics* printed in 1743, at a time when this question still agitated scholars. It seems that geometers today agree fairly unanimously on what we maintained then, that it is a dispute of words, and how could it not be one, since both parties are in complete agreement on the fundamental principles of equilibrium and movement. . . . The measurement of forces is therefore a question as useless to Mechanics as questions about the nature of extension and movement. . . . In the movement of a body, we clearly see only two things, "space and time"; it is from this idea alone that all the principles of Mechanics must be deduced and that we can indeed deduce them from.[38]

[35] Du Châtelet (1740, §588, pp. 446–447).
[36] D'Alembert (1758, xxii). Costabel (1951, 280) reminds us that 'dispute of words' as a description of the quarrel comes from Voltaire (1740, 68).
[37] D'Alembert (1758, xxiii–xxiv). [38] Diderot and D'Alembert (1751–1772, 7:114).

D'Alembert condemns the conception of force inherited from Leibnizian dynamics for aiming to work back, via the equivalence of cause and effect, from well-understood motion to the obscure motive causes that produce it. He proposes to stick to the motion itself, and to account for it with the three principles that explain all the laws of bodily motion: inertia, compound motion, and equilibrium.

The renewed centrality of motion is not a return to the Cartesian position. By tying force to the pillory of metaphysics, now considered useless and obscure, it leads to disregard for any consideration relating to the nature of physical entities. But by disqualifying the Leibnizian epistemic model, which grants force a fundamental metaphysical role, D'Alembert narrowed the concept of force to inertia alone, understood as a force that cannot fundamentally reside in bodies, and left otherwise unexplained. But, by his own admission, this refusal to engage with such questions inaugurated not so much a new era in which physics would finally be rid of metaphysics, as that of a science working uneasily with its indeterminacies.

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